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THE MOST ECONOMICAL POINT OF CUT-OFF.

By DE VOLSON WOOD.

In complying with Professor Marks' request in the last February number of this JOURNAL, I hope to remove the grounds for the imputation of any unfairness which he has been pleased to impute to me. In his communication of December last, equation (1) is

$$y = \frac{\text{Mean effective pressure} \times V}{\text{Cost steam} + \text{constant charges} - \text{value saved by compression}} \quad (1)$$

and equation (6) deduced from (1) is

$$y = \frac{eP_b\left[1+\log.\frac{1}{e}-\frac{k}{e}\right]-B\left[1-b\left(1+\log.\frac{b}{k}\right)\right]}{e+\frac{C}{D}\left\{eP_b\left[1+\log.\frac{1}{e}-\frac{k}{e}\right]-\left[B-b\left(1+\log.\frac{b}{k}\right)\right]-b\frac{B'}{P_b}\right.} \quad (6)$$

The second term of the denominator of equation (1) is *constant charges*, while the second term of the denomination of equation (6) is a function of e, a variable. This apparently changes the character of equation (1) and requires explanation. We find, however, if he explains that point, and if we admit his assumption that his "so called" constant charges are a fraction only of the cost of steam, that they

will disappear in determining the maximum of his expression as he claims; for it is equivalent to a function of the form

$$y = \frac{f(\varphi)}{F(\varphi) + af(\varphi) + c},$$

from which we find for a maximum, the relation,

$$[F(\varphi) + af(\varphi) + c]f'(\varphi) - f(\varphi)[F'(\varphi) + af'(\varphi)] = 0;$$

or

$$F(\varphi).\, f'(\varphi) + cf'(\varphi) - f(\varphi).\, F'(\varphi) = 0;$$

from which $af(\varphi)$ has disappeared; and it is apparent from the process that the only law which will cause the second term of the denominator of the fraction to disappear, is to make the ratio between it and the numerator constant. If this, then, were the only law, there would be no other solution. If there be any variation from this law, my conclusion, that the solution is *special*, is correct.

In the last February number, Professor Marks states: "That the constant charges are dependent solely on wages, cost of oil, interest, insurance, taxes and probable deterioration of machinery, and independent of all else."

"The person designing an engine knows what horse-power is expected of it."

On page 82, he says: "How is John Doe to get 150 horse-powers for the least sum of money?" "There is no answer save this: *by using the least possible steam per horse-power per hour.*"

On page 84: "I have assumed John Doe to be a manufacturer, who wishes to do that thing which at the end of one year or ten years is most profitable, and not a person cramped for capital."

The problem before us, then, is the *designer's* problem, and not the *owner's* problem.* John Doe having confidence in the Professor, secures his services in selecting the engine which will be the most economical for delivering 150 horse-powers for ten years. From the known initial pressure (say 90 pounds), the back pressure (say 5 pounds), and the clearance (which he assumes to be small), he finds by means of equation (8)

$$c = \frac{B}{P_b} = \frac{5}{90} = \frac{1}{18},$$

* The *designer's* problem consists in making an engine which will deliver a given number of horse-powers most economically; the *owner's* problem consists in delivering the greatest number of horse-powers from a given plant with the most profit to the owner. When the designer has properly solved his problem, it is merged into that of the owner's.

for the approximate value of the cut-off; and for a more accurate value

$$e = \frac{B\left[1-b\left(1+\log_{\cdot}\frac{b}{k}\right)\right]}{P_b} + k - b\frac{B}{P_b}\log_{\cdot}\frac{1}{e}, \qquad (7)$$

which for want of data we cannot reduce numerically, and hence will assume it as $\frac{1}{17}$, or even $\frac{1}{16}$. These men go into the market, and with the given data, the Professor selects the engine that will produce the 150 horse-powers "*by using the least possible steam per horse-power per hour;*" excepting that, for commercial reasons, he quietly uses a cut-off of $\frac{1}{10}$* as the basis of selection.

Doe learns from Smith, the proprietor, that the price is $4,000. Doe questions the wisdom of putting so much money into an engine for his business—at any rate, he intends to see if he cannot do better. Smith, desirous of securing a customer, after learning the capacity of the engine desired, offers one for $3,000, which he guarantees will deliver the required power. Mr. Doe then enters into a conversation with these men, which we may imagine runs as follows:

Doe. Do you think, Professor, that the smaller engine will do my work?

Professor. I have no doubt of it; but it will cost you more for steam, every day you use it, than the larger one; and I suppose you want to get your steam for the least money; and I have shown in my publications that the cheapest engine in the long run is one "*that will use the least possible steam per horse-power per hour,*" and the larger engine will do it, and the smaller one will not.

Doe. Steam! I have my boilers set, and they will furnish the steam; it is an engine I am now after.

Professor. Yes, my good sir, but do you not understand that to make steam requires fuel, and fuel costs money; and an engine that uses more steam than is necessary, wastes money; and although this appears evident, some writers "have deceived themselves, and perpetrated the absurdity of saying that you can save money by using more steam than is really necessary to do the work." (Feb., 1884, p. 83.)

Doe. My good friend, you may yet regret criticising those writers in this manner; for sometimes it is *economy* to *waste*. For instance, I could have saved $1,000 in building and stocking my shop had I been six months longer about it; but the interest alone would amount to

* JOURNAL FRANKLIN INSTITUTE, Dec., 1883, and Jan., 1884, p. 41.

more than that, and I expect to make twice that amount in that time. It is barely possible that those writers saw that by *losing* in the cost of steam they could *gain more* at some other point in this problem. But never mind that now, for we must select our engine. Please tell me how the smaller engine can do the work.

Professor. By cutting off later in the stroke. By following the piston with the full pressure of the steam further in the stroke, more power will be secured; but by so doing you will reject more heat in the steam at the exhaust, thus requiring a larger amount of steam, and as we have said, more fuel to do the same work; and I suppose you want to get your 150 horse-powers *"by using the least possible steam per horse-power per hour;"* and this will govern the point of cut-off, and consequently the proper size of the engine to be selected.

Doe. But this $1,000 difference in the first cost is too great for delivering the same power. Mr. Smith, I think your price of the larger engine is too high. Our Professor says: "The cost of an engine is not proportional to the size of the cylinder, but rather to the amount of labor put upon the engine as a whole. (February 1884, p. 84.)

Smith. The Professor is dealing in "general assertions;"* he ought to give "some law, equation, or example." He does not even intimate whether the work put upon an engine as a whole, in practice, bears any relation to the volume of the cylinder. Should I charge you $5,000 for the larger engine, it might not conflict with his statement. The fact is he denies the existence of one element of cost, but in the same sentence admits another element, which opposes his own denial. My experience in the business, and observation of general practice, enables me to say, confidently, that the cost of well-made engines varies with† the volume of the cylinder; that is, an engine with a large cylinder costs more than with a small one. I am able to refer you to some tabulated results on this point by Professor Trowbridge, in the Transactions of the American Society of Mechanical Engineers for 1882, page 231, and of remarks thereon by Professor Thurston, ibid, 1882, Vol. III, p. 289, showing that for engines above 50 horse-power, the cost is approximately as the volumes of the cylinders. The fact is, Professor Marks advocates a short cut-off, and requires a correspondingly large cylinder. You could probably get 200 horse-

* February, 1882, p. 81.

† In the January number I used *with* in the sense of a functional relation, without deciding whether the relation was direct or inverse. D. W.

powers out of the one he has selected, other things being properly proportioned.

Professor. All the dealers charge in that way, and I suppose we must submit to it.

Doe. The question of the cut-off grows in interest. I see our Professor says: "The point of cut-off has, practically, nothing to do with the constant charges, save so far as it determines the volume of the cylinder required."

Smith. I do not presume to understand all that is written about the cut-off; but there is something amusing in this statement of the Professor. As before, he denies one proposition, and then, in my opinion, admits another element which opposes his denial; for in my opinion, the size of the cylinder, involving as it does the cost of the engine, does affect the economical point of cut-off by making the constant charges, so called, different from what they otherwise would be; for the truth of which I appeal to you in the case of the two engines now before you. The consideration of the theoretical effect of constant charges, I leave to the Professor. There is another peculiar expression, "the volume of the cylinder is determined by the constant charges." Does the Professor come here with the constant charges known, and select a cylinder such that the interest, repairs, etc., thereon will produce these charges?

Professor. I should have said that the constant charges are dependent upon the volume of the cylinder.

Doe. Exactly. You have now touched a practical point; for I desire to make those constant charges, as you call them, as small as I can, for I will be obliged to pay for them in some way.

Professor. But remember that the 150 horse-powers will cost you more for fuel with the smaller engine.

Doe. How much more? I am willing to *lose* on the cost of steam, if I can save more on these constant charges. This $1,000 in first cost ought to have some influence on the ultimate economy.

Professor. It would seem so at first sight, but I have considered a case in my discussion of this subject. I observed that by saving $1,000 in first cost of the engine, you could afford to expend more money in producing steam; but to raise more steam, economically, would require larger boilers, and so there would be nothing saved. (Feb. 1884, p. 84.)

Doe. Now you have introduced another element into the problem. Does the capacity of the boilers affect the point of cut-off?

Professor. Not according to my theory, for they enter, if at all, as constant charges (the conclusion, however, being said hesitatingly after the remark above by Smith).

Doe. But it appears to me that you reached your result very abruptly. You speak definitely, of saving $1,000 and $3,000, and then quickly conclude that "we can safely set off against any possible saving effected by diminishing the size of the steam cylinder, the greater cost of increase, logically required, in the size of the boilers." (P. 84.) I see no figures to substantiate this conclusion; did you work it out thoroughly?

Professor. I did not compute it very accurately, but you will readily see that an engine requiring more steam may require larger boilers.

Doe. Not necessarily so, for it might be *economy* to *lose* on the fuel and save the cost of enlargement. Suppose that the interest, repairs, etc.—"constant charges"—are 16 per cent. on this purchase, I would save $160 per year if the smaller engine will answer. If my boilers are somewhat under size, perhaps $50 per year extra in fuel will supply the steam, in which case there will be a decided saving. Please examine these specifications of my boilers and inform me if they have sufficient capacity for the smaller engine.

Professor (after figuring). They will answer.

Doe. The boilers being out of the question, does it not follow, from your own reasoning, that it may, and probably will, be more economical for me to purchase the smaller engine.

Professor. Well, it appears so; but I still assure you that *the larger engine will give you the* 150 *horse-powers with less cost for fuel than the smaller one.*

Doe. My good friend, why do you continually bring to the front that proposition. I take a broader view of this transaction. It is not steam I want to save, but money. "I am a manufacturer, who wishes to do that thing which at the end of a year, or ten years, is most profitable" (p. 84); and your theories are not worth the paper they are printed on, if they tend to mislead me.

Professor. But I have shown in my writings that "your only opportunity *of saving money lies in saving steam per horse-power per hour.*" Here it is in the last February number of the JOURNAL OF THE FRANKLIN INSTITUTE, page 83.

Doe (reading). I see you make the assertion, but I do not see the argument. You have asserted that the saving in cost of engine must be expended in increased cost of boilers, or fuel, or both, but this needs confirmation; but, fortunately, in my case, the boilers are out of the question. Now are you not forced to admit that it will be economy for me to purchase the smaller engine, provided I save annually $160 in interest, repairs, etc., and expend $100 more for fuel, than I would if using the larger engine.

Professor. As you put it I must admit it.

Doe. But the smaller engine has a longer cut-off than your theory gives; must you not admit that these charges, of interest, repairs, etc. influence the most economical point of cut-off?

Professor. Well ———.

Doe. Never mind, it is not my object to embarrass your theories, but to select an engine.

Professor. But I question the accuracy of your estimate for extra cost of fuel.

Smith. Here are some figures taken from our actual practice.* To deliver 150 horse-powers at 80 lbs. boiler pressure, requires, at $\frac{1}{4}$ cut-off 17·7 lbs. of steam per horse-power per hour; at $\frac{1}{5}$ cut-off, 18 lbs. of steam; at $\frac{1}{10}$ cut off 25·3 lbs.

Doe and *Professor.* What! more steam for the shorter cut-off. Let us see the figures. (Musingly) that is so.

Doe. Then even the cost of steam is in favor of the smaller engine.

Smith. The difference is chiefly due to the condensation of the steam in the cylinder. Where expansion is 10 or more the condensation must be considerable, and I would not be surprised to learn that it even exceeded the largest figures here given.

Professor. This looks suspicious; for, theoretically, the engine of my selection should require only 13·8 pounds, and the smaller one 15·6 pounds.†

Smith. I have here some tables of condensations,‡ giving the latest, and perhaps the most reliable information on this point. The percentage of the whole consumption of feed-water at $\frac{1}{20}$ cut-off is 42; at $\frac{1}{10}$, 34; at $\frac{1}{8}$, 29; at $\frac{1}{5}$, 26; at $\frac{1}{4}$, 24. Will the Professor calculate what the actual consumption should be according to these figures?

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* Circular of Buckeye Engine Co.

† *Idem.*

‡ Barrus on Tabor's Steam Indicator.

Professor (figuring). At $\frac{1}{10}$ cut-off, 13·8 $\left(1 + \frac{34}{100-34}\right) = 20{\cdot}91$ pounds; at $\frac{1}{8}$ cut-off, 15·6 $\left(1 + \frac{26}{100-26}\right) = 21{\cdot}07$ pounds. This gives 20·91 pounds for the larger engine, and 21·07 pounds for the smaller; figures very different from those produced by Mr. Smith, and favorable to the larger engine.

Doe. How much will the difference amount to per year?

Professor. (21·07 — 20·97 = 0·16) × 150 HP × 10 hours × 300 days ÷ 1,000 = 72 thousand pounds of steam, which, at 25 cents per thousand, will be $18 per year. But the authors differ considerably in regard to the results of condensation. I see in Barrus' book, from which you took the above figures, that the difference in favor of the larger engine is for $\frac{1}{10}$ and $\frac{1}{8}$ cut-off, respectively, 0·97 of a pound of steam per horse-power per hour, making a difference of about $110 per year from this cause only.

Doe. I see that there is a possibility of considerable loss from this cause, but the most unfavorable showing does not equal the $160 saved in "constant charges," and I have decided to order the smaller engine.

Professor. But does not your view of the case warrant you in ordering a still smaller one?

Doe. Possibly; but the practical result is this: you selected an engine according to your theory; Mr. Smith selected another according to his general practice, and, by the help of both of you, I have satisfied myself that Smith's is the more economical engine of the two for me to purchase for my business. If another size were selected, we would apply to it the same mode of reasoning, and ascertain if the money *saved* by the interest, repairs, etc., equaled that *lost* by fuel and other conditions; and, by trying a few engines in this way, we would at last find one which, for such a cut-off as would deliver the required horse-powers with the given steam pressure, would be the most economical for me to purchase, considering first cost, cost of fuel and constant charges. Such an engine is said to have "the most economical point of cut-off." Your method, apparently, does not contemplate this process, and evidently needs revision.

The Professor retires, disappointed at the result. His rule had not purchased the engine, and his theory had received some fatal blows.

On his way he met a friend who had given some attention to the problem of the cut-off, and they entered into a conversation.

Professor. Would you believe it? John Doe has just purchased an engine for his shop that will cost him, say $100 per year more for steam than it would for a certain other engine which I selected for him.

Friend. I thought John Doe was a shrewd business man. How did it happen?

Professor. Smith convinced him that it would be more economical for him in the long run.

Friend. These salesmen are sharp; but what arguments did he use?

Professor. Why, he convinced him that his saving in interest, repairs, deterioration, oil and wages would exceed the extra cost for fuel.

Friend. Was not that correct?

Professor. Really, I could not gainsay it, with the conditions and figures they gave; but it gives a very different value for the cut-off than found by my theory. I have insisted that the "constant charges" disappear in determining the proper point of cut-off; but according to their showing they have considerable influence. Doe's boilers are already set, and, fortunately for him, they prove to be large enough to furnish the required steam economically. Had they been much smaller, he would have been obliged to have taken the engine of my selection, or expended all he saved on the engine in putting in larger boilers.

Friend. Exactly, and I have noticed your difficulty on this point. The problem is one of maxima and minima, and the fact is you have assumed, beforehand, certain quantities as constant, which, in the process of designing, are variables; and these you enter in your function before seeking the maximum, which, as a mathematical operation, you know, even better than I, to be erroneous, and will fail to give the true maximum. Your admission just made in regard to the boilers is fatal to your theory; for it shows that, in designing an entire plant, the most economical point of cut-off will involve the first cost of the boilers while you declare it to be independent of all such considerations. You have just seen, also, that after they are set, it has had its influence in selecting the engine. Again, you say "C = the constant charges in dollars and cents, and is assumed to remain constant after once being fixed upon." (Feb., 1884, p. 85.) Please inform me

how you could fix the charge for interest before you knew the cost of the engine.

Professor. I assumed that "the constant charges may be determined regardless of the cost of steam." (Feb., 1884, pp. 83 and 84.)

Friend. The answer is rather evasive; for I referred to the cost of engine—not of steam; but, as they are in a measure related, I will say that your assumption is not admissible. The smaller the engine, the less the first cost, and the interest on the money correspondingly less; while the cost for steam will be more so that this quantity—which you have considered constant—*varies* inversely with the cost of steam. Similarly, the cost for repairs and deterioration *vary* in the same sense but not in the same ratio, nor necessarily according to the same law. After the engine is set, the interest is *fixed*, whether the engine be run 200 or 300 days, or if it be run night and day; while the repairs will depend chiefly upon the amount it is used. I am aware that the problem involving such contingencies cannot be solved, but I mention them to show more clearly, if possible, that the proper point of cut-off is not independent of these things.

Professor. But you must admit that after the engine is set, all the charges which can be considered constant are fixed, and must be some "fraction of the mean effective pressure." That John Doe, *after* "his purchase and arrangements are made, has done with constant charges, and must look to economy of steam as the only means of saving money." (Feb., 1884, p. 83.)

Friend. I think our friend, John Doe, will not agree with you. He will inform you, even after his engine is set, that he "has not done with constant charges," that he finds a continual draft on his purse for interest, repairs, oil, insurance and taxes; and that he has no commendation for the man who fixes one dollar upon him unnecessarily. True, if he has been mislead in his purchases, and been made to pay an unnecessary amount in first cost, he will be obliged to endure it; but it is wiser to consider all these elements *before* the purchase. But your plea, that they become fixed *after* the purchase, is fallacious from a mathematical point of view; you might as well argue that the cut-off becomes *fixed* after you have found it, or that any other element is fixed. It is unnecessary to argue this point; it is only necessary to convince you that these so-called "constant charges" are variables in the process of designing, to cause you to abandon your rule, giving as it does a cut-off of $\frac{1}{10}$ to $\frac{1}{20}$, which you have insisted up to the present

time to be "practically accurate within the widest range," after establishing certain relations in regard to the steam. (Jan., 1884, p. 1.) When thus convinced, you will admit that "economy of fluid" is only one element of the general problem of economy; that by saving steam *only*, you will lose money. You will then see that your arraignment of living and deceased writers is premature, to say the least. You will also find that you expose yourself to attacks by critics; for instance, you stated that "the volume of the cylinder is dependent upon the constant charges," that "the cut-off is independent of those charges"—conditions incompatible; that whenever it is possible to fix the constant charges on and to design an engine regardless of its horse-power, then will the proof lack in generality" (p. 82), an admission which is fatal to your theory. For in the next paragraph you assert that several of the charges are independent of the horse-power within certain limits, and that for these at least the proportion on page 86 cannot be true; but, even if true for these, the same proportion cannot be true for the other charges, as interest, for instance.

Finally, you are already aware that Rankine made the most general solution yet attained, in which the several elements above mentioned are treated as variables; a special case of which includes your problem of "economy of steam only," and of which the general case includes both the designer's and owner's problems. You are also aware that Denton and Wolff unearthed this solution, so to speak, enlarged upon and amplified it, by discussing it in their papers and applying the method to a large number of existing engines;* and, although it is not claimed by these writers that the method possesses mathematical exactness, yet it is claimed that the defect in the process lies chiefly in the data rather than in the method. In view of these facts, you may at last consider it advisable to take to yourself the advice you have given to another, and "give the most careful consideration to the point at issue."

[NOTE.—Professor Rankine stated his problem thus: "By increasing the ratio of expansion in a Cornish engine, the quantity of steam required to perform a given duty is diminished; and the cost of fuel and of the boil-

† Transactions of the American Society of Mechanical Engineers, 1881, page 147 and page 281; republished in the *American Engineer*, 1881, June, July, August and November; and in the Transactions of the Institution of Civil Engineers (London), 1881-82, Vol. LXVIII, Part II, p. 75; Part III, p. 44.

ers is lowered. But at the same time, as the cylinders and every part of the engine is increased, must be made larger to admit of a greater expansion, the cost of the engine is increased. It thus becomes a problem of maxima and minima to determine what ratio of expansion ought to be adopted under given circumstances, in order that the sum of the annual cost of fuel, and the interest of the capital employed in construction may be the least possible as compared with the work done."—*Phil. Mag.*, 1854, (2), p. 345.

This problem Rankine solves in his usual masterly manner, and then shows how to design a 100 horse-power engine, according to his method, making the cost of engine and of boilers variable; deducing the point of cut-off, the area of the piston, and mass of the boilers. In this he uses the graphical method. In the owner's problem, the cost of the plant being known, the interest will be known, and the cost of fuel and point of cut-off are the variables, but the *method* of finding the point of cut-off is the same as before.]

Stevens' Institute of Technology, Hoboken,
February, 15, 1884.

Critical Point of Liquefiable Gases.—Jamin believes that gases are liquefiable at every temperature, when the pressure is sufficient. He defines the critical point as the temperature at which a liquid and its saturated vapor have the same density; but the general law of vaporization is not suddenly interrupted; the liquid continues to be at its point of ebullition and maximum tension. If it ceases to be visible it is because it is mixed with the gas, in which it swims on account of the equal densities, and when the temperature continues to increase the tension also increases until the liquid is entirely volatilized. Then the space ceases to be saturated and there is only a dry vapor, or gas, far removed from its point of liquefaction. In general, a saturated vapor is distinguished from its generating liquid by its smaller density and its latent heat; in the present case the densities are equal and there is no latent heat, since the volume does not change and there is no work of expansion. Hence, at the critical point, nothing distinguishes the liquid from its vapor, neither the tension, the density, the constituent heat, the appearance, nor any other known property. Jamin submitted these views to Cailletet, with the statement that if his hypothesis was true the liquefaction would be retarded by replacing the air in his mixture of air and carbonic acid by some lighter gas, for instance, hydrogen. Cailletet performed the experiment and found that the results were such as had been predicted.—*Comptes Rendus*, May 21, 1883.

THE ALLEGED "REMARKABLE ERROR IN THE THEORY OF THE TURBINE WATER WHEEL."

By I. P. Church,
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Though it can hardly be urged that any theory of turbines has been confirmed by experiment with precision, or has even had more than a slight influence on their design and operation, it is nevertheless a matter of importance and justice that the "common theory," presented by Weisbach and Bresse, should be recognized as employing sound principles of dynamics in obtaining results from its fundamental hypotheses, whether the latter are perfectly realized in practice, or not. Let us undertake a brief sketch of this theory, omitting all considerations of frictional and other resistances, for the sake of simplicity. Its fundamental hypotheses are: "steady motion" of the water, uniform angular velocity of the wheel, "flow in plane layers," filled channels, "full gate," and no shock on entering the wheel (*i. e.* at entrance the relative velocity of the water must be tangent to the wheel float; otherwise there would be an abrupt change in the absolute velocity, rendering incorrect the assumption to be made here that the internal pressure, p_1, at the beginning of the wheel channels is the same as that toward the extremity of the guide passages).

The relations holding good in these hypotheses between the quantities involved are stated in equations (1) to (6) inclusive; in which Q denotes the volume of water used per second; h, the height from head to tail water; h_2, the mean depth of wheel channel below surface of tail water; F, F_1, and F_2, the total cross sections of the ends of guide passages, and at entrance, and at exit, of wheel channels, respectively; c_1, r_1, v_1, the relative velocity of water, the wheel radius, and the wheel rotary velocity at the entrance of a wheel channel; c_2, r_2, v_2, corresponding quantities at exit of wheel channel; p_1 and p_2, the mean internal water pressures at entrance and exit of wheel; w, the absolute velocity at exit; γ, the weight of a cubic unit of water; b, the height of a water barometer; and g, the acceleration of gravity. We have then: energy transmitted to wheel:

$$= Q\gamma h - Q\gamma \frac{w^2}{2g}. \qquad (1)$$

Relation holding good for the *fixed* channels between the head water

and the extremity of guides where the absolute velocity is c, (Bernoulli's theorem):

$$\frac{c^2}{2g} + \frac{p_1}{\gamma} = h + h_2 + b. \tag{2}$$

Continuity of flow, channels filled:

$$Q = Fc = F_1c_1 = F_2c_2. \tag{3}$$

Trigonometrical relations in the parallelograms of velocities at entrance and exit from the wheel. (4)

The proportion $v_1 : v_2 :: r_1 : r_2$. (5)

Relation between the relative velocities and internal pressures of the water, and the rotary wheel velocities at entrance and exit of a wheel channel $\left(\text{assuming } \frac{p_2}{\gamma} = h_2 + b\right)$, viz.:

$$\frac{c_2^2}{2g} + h_2 + b = \frac{c_1^2}{2g} + \frac{p_1}{\gamma} + \left[\frac{v_2^2 - v_1^2}{2g}\right] \tag{6}$$

Weisbach's presentment of this last relation is marred by the statement that it is connected with some mysterious "centrifugal force." He does not prove equation (6), but thinks it sufficient to base it upon the result

$$c_2^2 - c_1^2 = v_2^2 - v_1^2 \tag{7}$$

obtained in the problem of a rotating top or disc, in a channel on whose surface a small solid body is free to move without friction (or gravity, if the groove is horizontal). (Vol. I, §§ 303 and 304.)

Equation (7) is correct, and will be demonstrated further on, as well as (6), which does not depend directly on (7), as Weisbach implies.

An article by Mr. Frizell in the number of this JOURNAL for August, 1883, entitled "Remarkable Error in the Common Theory of the Turbine Water Wheel," by implication denies the truth of (7) on account of an absurdity in Weisbach's method of demonstrating it. The ideas and method of Weisbach's treatment of that problem have already been referred to as absurd, in an article by the present writer, prepared five years ago (see p. 215 of Van Nostrand's Eng. Mag. for March, 1880); but it is clear that Mr. Frizell does not appreciate all the absurdities of the situation, since he upholds Weisbach's ideas in the particular case where the groove is radial.

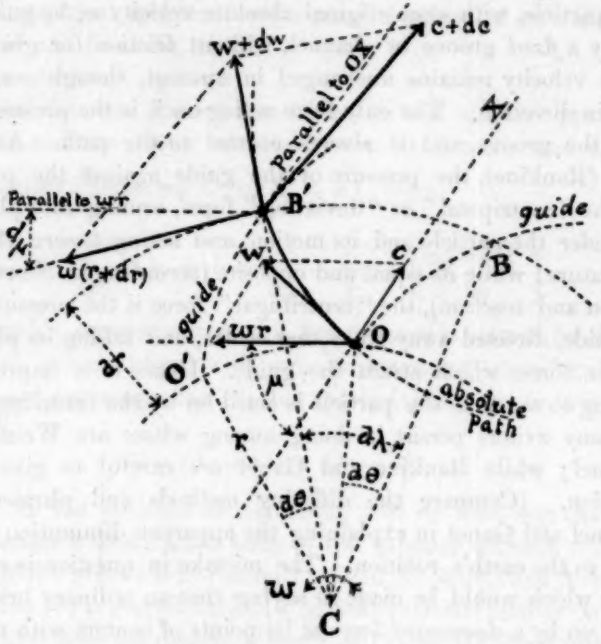
Since Weisbach's method of demonstrating equation (7) is certainly to be condemned, let us apply to the problem of the disc correct prin-

ciples of dynamics (viz., Newton's laws), employing no conceptions beyond those of time, space, mass, and force, omitting all use of the compound ideas of work and energy, which always owe their meaning, and the means of verifying results obtained by them, to the former conceptions. As a preliminary, however, it will be useful to glance at a more familiar and simple problem in which the use of the terms "centripetal" and centrifugal" have no ambiguity.

If a particle, with some original absolute velocity w, be guided in a curve by a *fixed* groove or channel, without friction (or gravity), its absolute velocity remains unchanged in amount, though continually altered in direction. The only force acting on it is the pressure of the side of the groove, and is always normal to the path. As is well known (Rankine), the pressure of the guide against the particle is called the "centripetal" or "deviating" force, coming into play when we consider the particle and its motion, and acting *toward* the centre of curvature; while its equal and opposite (according to Newton's law of action and reaction), the "centrifugal" force is the pressure on the fixed guide, directed away from that centre, and taking its place with the other forces which strain the guide. Hence it is improper and confusing to say that the particle is acted on by the centrifugal force, as so many writers persist in doing, among whom are Weisbach and Deschanel; while Rankine and Ganot are careful to give no such impression. (Compare the differing methods and phraseology of Deschanel and Ganot in explaining the apparent diminution of gravity due to the earth's rotation. The mistake in question is similar to the one which would be made in saying that an ordinary bridge truss is acted on by a *downward* force at its points of contact with the piers; a mistake, however, never made; the occurrence of the other is therefore surprising.

Next, suppose the curved groove or channel to *rotate*, with a uniform angular velocity ω, about a fixed axis perpendicular to its plane, while the particle is passing through it. Since there are now, at any instant of its progress, *three* different centres to be considered, viz., the fixed centre of the disc, the centre of curvature of the groove, and that of the absolute path, for the point of contact, it is best to drop all such terms as centrifugal, or centripetal, force, or "centrifugal action," as their use in this connection tends to ambiguity and vagueness. As before, the only force acting on the particle (gravity is eliminated, since the disc is horizontal) is the pressure of one side of the guide

against it, and must be normal to the guide curve (there being no friction), *i. e.*, is directed toward the centre of curvature of the latter. This force, not being in general normal to the absolute path, causes a continual change in the absolute velocity. The absolute velocity w, is the diagonal formed on the rotary velocity $v = \omega r$ of the point of contact of guide, and c the velocity of the particle relatively to this



point of the guide, r being the radial distance of the particle, at any instant, from the centre of disc. It is required to find the law of variation of c, the relative velocity, its value being c_1 at a definite point of the groove where $r = r_1$.

Let OB' represent any element, described in a time dt, of the absolute path; $\mu =$ the angle between the rotary velocity ωr and the guide tangent at O. During the time dt the angular motion of the disc is $d\lambda$, the relative angular motion of the particle, $d\theta$; r has increased an amount dr. From kinematics and trigonometry the following relations are evident:

$$OB, \text{ or } O'B', = cdt$$

$$\omega = \frac{d\lambda}{dt} \tag{8}$$

$$cdt \sin. \mu = dr \tag{9}$$

$$cdt \cos. \mu = r\, d\theta \tag{10}$$

and
$$d\mu = -(d\lambda - d\theta) \tag{11}$$

Since the only force acting on the particle at O is perpendicular to OX (call this the axis X), it is incapable of affecting the X component of w, *i. e.*, w_x, which = the algebraic sum of the X components of c and ωr. Hence we must have

$$\frac{dw_x}{dt} = \text{zero, } i.\ e., \frac{d\,(c - \omega r \cos. \mu)}{dt} = 0$$

or, in detail, ω being the only constant,

$$\frac{dc}{dt} - \omega \cos. \mu \frac{dr}{dt} + \omega r \sin. \mu \frac{d\mu}{dt} = 0$$

Substituting from (11), we have

$$\frac{dc}{dt} - \omega \cos. \mu \frac{dr}{dt} + \omega r \sin. \mu \frac{d\theta}{dt} - \omega r \sin. \mu \frac{d\lambda}{dt} = 0 \tag{12}$$

Transforming the second term of (12) by the aid of equation (9); the third by (10); the fourth by (8), then by (9); we finally obtain

$$\frac{dc}{dt} - \omega c \cos. \mu \sin. \mu + \omega c \cos. \mu \sin. \mu - \frac{\omega^2 r dr}{cdt} = 0 \tag{13}$$

$$i.\ e., \frac{cdc}{dt} = \frac{\omega^2 r dr}{dt}; \text{ whence } cdc = \omega^2 r dr,$$

which integrated between corresponding limits, c_2 and c_1 for c, r_2 and r_1 for r, ω being constant gives

$$\frac{c_2^2 - c_1^2}{2} = \frac{\omega^2 (r_2^2 - r_1^2)}{2} \tag{13a}$$

i. e., $c_2^2 - c_1^2 = v_2^2 - v_1^2$, *Q. E. D.*; a result independent of the form of the guide.

(The foregoing demonstration is a reproduction, in a more compact form, of a portion of the article referred to in Van Nostrand's Mag.)

The errors in Weisbach's treatment of the problem can now be better appreciated. He makes the pressure or force acting on the particle at any instant radial to the disc axis, whatever the form of the

groove, instead of normal to the groove, and assigns to it a value $\omega^2 Mr$, also independent of the form of groove, whereas the true value of that pressure is

$$N = M\left[\frac{c^2}{\rho} + \omega^2 r \cos.\ \mu - 2\omega c\right] \qquad (14)$$

M denoting the mass of the particle.

(From the same article in Van Nostrand's Mag.) In (14) ρ is the radius of curvature of the guide curve at the point of contact. If the groove were radial to the disc, μ would $= 90°$ at all points, $\rho = \infty$ hence the pressure against the particle is numerically $= 2M\omega c$, and is perpendicular to a radial direction. Again, if the groove were circular and concentric with the disc, and the particle stationary in space, we have at every point of groove, $c = \omega r$, $\mu = 0°$, and $\rho = r$, whence N reduces to zero, as it should, since there is no action between the disc and particle. Also, with a circular groove, concentric with the disc, the particle having an absolute velocity w, we have, as before, $\mu = 0°$ and $\rho = r$, while $c = \omega r - w$; which reduces N to $Mw^2 \div r$, the "centripetal force," or pressure, from a fixed circular guide; which again testifies to the correctness of the expression for N in (14); for in this case, evidently, the constraint of the groove on the particle is the same as if the former were fixed.

Weisbach's expression $\frac{G}{g} \cdot \frac{c_2^2 - c_1^2}{2}$ which he calls "energy stored," and in which c_2 and c_1 are relative velocities, does not represent work done upon the particle, since (see (13a)) it is independent of the form of the path between the initial and final circumferances, whereas the pressure N is not. For example, it would be possible to so design the groove as to allow the particle to traverse the disc without contact with either side of the groove, hence without pressure, and rectilineally. In this case the particle has "true angular velocity" about the disc centre, but what becomes of either Weisbach's, or Mr. Frizell's "centrifugal force?" The equation of such a curve is determined from the condition $N = o$ at all points. In their demonstration Weisbach and his copyists were probably misled by the brevity and simplicity of its algebraic matter and the current misuse of the term "centrifugal force."

Resuming the "common theory" of the turbine, it is hardly sufficient to base (6) on (7) by mere inference. The variation of c, the

relative velocity of water traversing and filling a uniformly rotating passage or pipe, when "steady motion" (or a "state of permanency) is once established, is, however, easily deduced from the same figure. By "steady motion" it is implied that the mean internal pressure and velocity of the water at any section of the passage are always the same for that part of the passage while the flow is going on; *e. g.* the same pressure which acts on a retarding force at one end, B, of a lamina whose thickness $= OB = cdt$ (and whose base is F, normal to OB) becomes an accelerating force for it when the base, originally at O, has arrived at B', dt having elapsed. Let $p =$ the mean internal water pressure at O, then $p + dp =$ that at B (or B'). Hence, since the pressure on the edges of the lamina are all normal to OX, the sum of the X components of all the forces acting on the lamina is $= Fp - F(p + dp)$ *i. e.* $= -Fdp$, and this must equal the mass, $\frac{Fcdt\gamma}{g}$, multiplied by the X acceleration, *i. e.*, by $\frac{dw_x}{dt}$ which we have already in the left hand member of equation (13).

Therefore,

$$-Fdp = \frac{Fcdt\gamma}{g}\left[\frac{dc}{dt} - \frac{\omega^2 r dr}{cdt}\right];$$

whence

$$\frac{1}{g}cdc - \frac{\omega^2}{g}rdr = -\frac{1}{\gamma}dp,$$

in which the variables are separated. Integrating between any two points of the channel, as 1 and 2, remembering that γ, ω and g are constant, and that $\omega r = v$, we have, after transposition,

$$\frac{c_2^2}{2g} + \frac{p_2}{\gamma} = \frac{c_1^2}{2g} + \frac{p_1}{\gamma} + \frac{v_2^2 - v_1^2}{2g} \qquad (15)$$

Hence equation (6) is correct.

As to testing the "common theory" as here presented, by the valuable experiments of Mr. Francis on the Tremont turbine, it must be remembered that two of the hypothesis on which it rests are "full gate," and "no shock" at the entrance of a wheel channel.

Experiments Nos. 20, 21 and 22, realize the first completely, and the second very nearly; for in Exp. 21 the discharge being $Q = 139{\cdot}90$ cubic feet per second, we have $c = Q \div F = 21{\cdot}42$ feet per second, and $c \cos. \alpha = 19{\cdot}08$, which is very near the value for v_1 (19·13) found

by experiment. (The value $a = 27°$, the angle between c and v_1, was obtained from the drawing in Mr. Francis' book, and since the entrance float-tangent is perpendicular to v_1, we must have $v_1 = c \cos. a$ if c_1 is to follow that tangent.) Proceeding, then, with Exp. 21, as almost the only one fitted for the purpose, we substitute the values $F = 6{\cdot}53$ square feet; $F_1 = 19{\cdot}78$ (partly from the drawing); $F_2 = 7{\cdot}65$; $v_1 = 19{\cdot}13$ feet; $v_2 = \frac{4{\cdot}06}{3{\cdot}38} \times v_1 = 1{\cdot}201 \times 19{\cdot}13$ (*i. e.* v_2 is taken at the middle of the cross section of an exit orifice, as it should be); $h = 12{\cdot}899$; and $g = 32{\cdot}2$; in the formula

$$\frac{c_2^2}{2g} = \left[h + \frac{v_2^2 - v_1^2}{2g}\right] \div \left[1 + \left(\frac{F_2}{F}\right)^2 - \left(\frac{F_2}{F_1}\right)^2\right] \qquad (16)$$

obtained by elimination between equations (2), (3) and (6), and obtain $c_2 = 21{\cdot}16$ feet per second; and therefore the discharge $= F_2 c_2 = 7{\cdot}65 \times 21{\cdot}16 = 161{\cdot}87$ cubic feet per second. The observed discharge was 139·90, which is therefore 13½ per cent. less than the theoretical; but when it is remembered that in this presentment *no allowance whatever* has been made for "losses of head" due to any cause, nor for any deviation from the hypothesis at the base of the theory, this result is not to be regarded as unsatisfactory, nor as indicating error in the application of analysis in developing results from the hypothesis.

There is considerable variation in the sizes of the various cross sections, as in Venturi's tube, but the discrepancies (some of which are as high as 80 per cent.), recorded in Mr. Francis' book, between experiment and theory, in the case of the latter apparatus, have not as yet led any one to assert that the simplest formula of hydraulics, $v = \sqrt{2gh}$, is not "well founded in theory."

Ithaca, December, 1883.

Galvanizing of Brass and Iron.—Neujeau and Delaite, of Liége, have contrived a galvanizing process for objects of large dimensions or impossible to be removed, and which, consequently, cannot be plunged into a bath of melted zinc. The process is also cheaper than ordinary galvanizing and equally durable. Finely powdered zinc is mixed with oil and siccative. A varnish is thus formed, which may be applied with a brush in the ordinary manner. A single coat is often sufficient, but it is better to use two. The objects are then of an iron-gray and they can be left in that condition or bronzed or painted.

Chron, Industr., April 8, 1883. C.

PETROLEUM AS A SOURCE OF EMERGENCY POWER FOR WAR-SHIPS.

By N. B. Clark, Passed Assistant-Engineer, U. S. N.

A war-ship requires two different rates of speed, one, which for convenience of expression, may be called passage power, would be used on all ordinary occasions when steaming from port to port; and the other, emergency power, required for chasing an enemy or escaping from a superior force, when a high rate of speed will be necessary.

The requirements of a cruising war-ship and a commercial vessel making regular passages from port to port are entirely different. The passenger or fast freight steamer needs sustained high speed to enable her to make trips in the shortest time possible and with the utmost economy of fuel in order to pay a profit to her owners; while the war-ship does not need sustained high speed, but requires a still higher rate of speed to be used for only a few hours at a time in an emergency, it being admissible to attain this extreme high speed at an extravagant cost of fuel, as economy of fuel can only be attained by great weight of machinery, involving increased displacement.

It was the experience of officers who served on the vessels blockading the southern coast that if a blockade-runner was sighted early in the day her capture was almost a certainty notwithstanding the assumed superiority in speed of that class of vessels; but if the vessel was not sighted in time to admit of her capture before night, darkness frequently enabled her to elude her pursuers, even though they possessed superior speed.

As darkness favors the weak in eluding pursuit, commerce destroyers should be provided with high emergency power to enable them to capture their prizes while daylight lasts.

The passage power of a war-ship should be so designed as to be capable of being used with great economy of fuel, enabling the vessel to steam great distances, and to keep the sea for lengthened periods of time; while on the other hand, to avoid excessive weight of machinery, economy of fuel would be a matter of secondary consideration when using the emergency power, which would be but rarely called into action, and then for only a short period of time, and therefore would not warrant the encumbrance of a great weight of machinery.

The passage power of a war-ship may be sufficient to drive the ship

nine or ten knots per hour, while her emergency power should be equal to the attainment of double that speed. This would require the emergency power to approximate to eight times the passage power.

Such enormous power cannot be attained in a vessel of ordinary size, burning solid fuel on grate bars with natural draught, designed for economy of fuel, unless the entire hull of the ship is filled with boilers, absorbing the greater part of the displacement by the weight of the engines, boilers and fuel, thereby depriving the vessel of offensive and defensive power.

It has been proposed to construct such vessels, in which all other desirable qualities would be sacrificed to extreme high speed, but the propriety of such a course may well be questioned. High speed alone, without a due complement of defensive and offensive power, would simply enable a naval commander to chase down an enemy which he dare not fight, a feat by which he would gain only negative renown.

Although it is impracticable to construct a vessel of ordinary dimensions, combining extreme high speed with due defensive and offensive qualities, with motive power derived from the combustion of solid fuel or grate bars in ordinary boilers with natural draught, yet, with liquid fuel, such a speed can be developed in a vessel of very moderate displacement.

In boilers for consuming solid fuel the steam generating power is measured by the area of the grate surface, and even when the draught is forced, the amount of heat produced from such fuel is much less than the heating surface will absorb, provided the heating surface is the maximum the boilers will contain.

Solid fuel is burned only from its surface by the erosive action of diluted oxygen, consequently the combustion of such fuel is slow and torpid compared with liquid fuel, which can be converted into a gas with rapidity and facility.

Ordinary marine boilers for consuming anthracite coal have from 20 to 25 square feet of heating surface to the square foot of grate, and such boilers are found to give good results with solid fuel, as the heating surface is sufficient to absorb all the heat that can be generated from such fuel; but it is possible to construct tubular boilers for burning petroleum having 75 square feet of heating surface to the square foot of grate.

With such boilers having the liquid fuel sprayed into their furnaces by jets of superheated steam or hot air, the steam generating power

would be measured by the largely increased extent of the heating surface, and not by the limited area of the grate, as the fuel could be consumed at a rate fully up to the capacity of the heating surface to absorb the heat generated by its combustion.

In a properly constructed furnace, petroleum can be burned entirely without smoke, the combustion being complete, and its practical calorific value has been proved to be fully equal to three times its weight of the best coal.

A high authority on engineering subjects, *Molesworth's Pocket-book*, p. 460, revised edition, gives the following account of the method of burning petroleum, as practiced in England, with its advantages as a fuel:

"No alteration of the ordinary furnace or grate is necessary. For burning oil the grate bars are covered with slabs, overlaid with fine cinders, and the ash-pit doors closed. The oil fell vertically, a jet of superheated steam met it, and turned it into vapor, which then took fire and was consumed in a perfect manner.

"The water evaporated amounted to 20·8 pounds per pound of oil consumed. The average result of several days' experiment was 19½ pounds of water evaporated per pound of oil.

"With the best Aberdare coal the same boiler evaporated 6½ pounds of water per pound of coal consumed. The advantages claimed for liquid fuel in seagoing vessels are:

"1st. A reduction of weight of fuel.

"2d. A reduction of bulk of fuel.

"3d. A reduction of fire-room force in the porportion of 4 to 1.

"4th. Prompt kindling of fires.

"5th. The fires can be extinguished instantaneously.

"6th. Capability of stowage in place of water ballast, by which it may be replaced as consumed, and great facility for taking in rapidly.

"7th. Its cleanliness, and freedom from ashes cinders, etc.

"8th. The absence of the loss of heat due to the frequent opening of furnace doors.

"9th. The ability to command a more intense fire, and management of temperature without forced draught.

"10th. Facility for perfect combustion and rapidity of raising steam.

"11th. Freedom from smoke."

Mr. Henry F. Hayden, of Washington, D. C., has recently obtained

several United States patents on furnaces for an improved method of burning hydrocarbons, in which the liquid fuel is sprayed into the furnace by a jet of steam superheated to 1,200° Fahr., and having the air-supporting combustion heated to 800° Fahr. By this method it is claimed petroleum will give a calorific value greater than the above estimate.

In order to show the merits of petroleum as a source of emergency power we will take for illustration the proposed 3,000-ton cruisers, in which it is understood 1,200 tons of the displacement is allotted to steam machinery and fuel, the weight of the machinery being 700 tons, and the coal 500 tons.

If the boilers of these ships were specially designed to burn petroleum as an emergency fuel, their steam generating power could be doubled, while their weight could be decreased 150 tons.

The same boilers could also be used to consume anthracite coal with great economy when the ship was using her passage power.

If instead of carrying 500 tons of coal, the ship was equipped with 260 tons of coal, and 80 tons of petroleum stored in the cellular bottom, aggregating 340 tons, she would have her full complement of fuel, the equivalent of 500 tons of coal, thereby effecting a saving in weight of 160 tons.

If this aggregate weight of 310 tons, saved from boilers and fuel, was put into deflecting armor and heavy guns, in addition to the weights already allotted for that purpose, it would produce vessels of moderate size and cost, having a greater emergency speed than any existing commercial vessel, and having offensive and defensive powers equal to a heavy iron-clad. Such a ship would combine all the desirable qualities of a light, rapid cruiser, and a heavy coast-defence vessel.

The reason why petroleum has not been used as a fuel in the merchant marine is on account of its cost, and it is not likely that it will ever be able to compete successfully with its powerful rivals, anthracite and bituminous coal.

The objection to the use of petroleum in the vessels of the navy is its assumed dangerous character, but it should be remembered that the same objection could be urged against gunpowder, and was strongly urged against the introduction of steam, notwithstanding which both gunpowder and steam have been introduced, and will be retained in spite of their dangerous character.

In regard to the dangerous qualities of petroleum it may be said, if

its storage and use were surrounded by the same safeguards and precautions as those we observe in the storage and use of gunpowder, it would not be found any more dangerous. Besides it should not be forgotten that fighting, the purpose of a war-ship, cannot be made a safe business. The chief danger from petroleum arises from the emanation of an inflammable gas which is given off at all ordinary temperatures, but with refined petroleum this gas is scarcely appreciable in quantity when the fluid is kept at the temperature of sea water, which could be accomplished by storing it in the double bottom of the vessel, and danger from an accumulation of gas in the petroleum tanks could be avoided by providing them with appropriate ventilating pipes, leading overboard, above the water line.

While the cost of petroleum will bar its use as a fuel in the merchant marine, its introduction as an emergency fuel for the navy would be a measure of great economy. In the navy the neccessities of the service require a very large fire-room force, fully 33 per cent. of which may be denominated emergency men, whose services might be dispensed with except when the ship is using her full steam power.

With petroleum as a source of emergency power the services of these extra men could be dispensed with, and the cost of their pay, rations, etc., would far more than compensate for the difference between the cost of coal and petroleum, as the pay, rations, etc., of the emergency men would be continuous, while the extra cost of the emergency fuel would only have to be borne for short durations of time at long intervals.

One advantage to be derived from the use of petroleum as a source of emergency power is that it will enable us to retain anthracite coal as the standard fuel of our navy, otherwise we will be forced to use bituminous coal in order to compete in speed with the ships of other nations.

It is proposed to use anthracite coal as the source of the passage power, reserving the petroleum for emergencies, the same boilers serving for each, but of course the petroleum could be utilized for the lower rate of speed should necessity require it.

Numerous trials of single and twin screw ships of the British navy prove that twin screws utilize 11 per cent. more power in propelling the ship than single screws; this is no doubt due to the greater immersion of the effective area of the twin screws. The principal source of loss with the screw propeller is from skin friction, which is

constant at all depths of immersion, while the propulsive efficiency increases with the depth of immersion. As twin screws give an increased efficiency of 11 per cent. over single screws, it is highly probable that triple screws would show a greater efficiency than twin screws, as, owing to the deeper immersion of the shafts, a less area of blade would give an equal propulsive efficiency, and if there was less area of blade there would be less loss from skin friction, and consequently a greater proportionate efficiency from the power applied.

The application of two or more screws would result, not only in a greater economy and efficiency of the power applied, than can be obtained from a single screw, but also in greater safety to the vessel, as it is not probable that all the screws would be disabled at one time. It would also admit of a further decrease of spars and sails, which would be a great encumbrance to a vessel chasing a more lightly sparred adversary, who would not fail to run to windward.

In order to avoid unnecessary friction it would be desirable to disconnect a part of the machinery when using her lower rate of power. This could be accomplished by dividing the motive power of a twin screw ship between two vertical cylinders, direct acting, compound engines, one double cylinder compound engine being set forward of, and the other aft of a driving pinion on the crank shaft of each screw, with a disconnecting device to each engine. This arrangement would permit either or both compound engines to be used for driving the screw. When running with the passage power only one engine would be used to drive each screw, when using the emergency power both engines would be connected. This type and disposition of machinery would be a great safeguard against accidents.

According to the able and very valuable report of Passed Assistant-Engineer John A. Tobin, U. S. Navy, published as House Executive Document 48, of the Second Session of the 47th Congress, the weight of the steam machinery of British merchant steamers is 480 pounds per indicated horse-power, and that of the vessels of the British Navy is 289 for the light steamer *Iris* and 360 pounds for other vessels; the weight of the steam machinery of the torpedo ram *Polyphemas*, having locomotive tubular boilers, is 180 pounds; and that of the two classes of light swift torpedo boats is 57·7 and 66·5 pounds per indicated horse-power.

This great reduction of weight is accomplished by the adoption of the locomotive tubular boiler, constructed of steel, furnishing steam of

high pressure to engines constructed of the very best material, to secure great strength with lightness, designed for extreme high piston speed, whereby great power is transmitted by very light machinery, the light machinery developing the great power by its rapidity of movement.

If the same general plan of steam machinery, in a modified form, were adopted for cruising vessels as that applied in the construction of torpedo boats, engines capable of transmitting the proposed emergency power of 18 or 20 knots per hour could be constructed within the limits of weight allotted for that purpose.

As the torpedo boats referred to, having a displacement of less than 100 tons, have attained a speed of 22·4 knots per hour on the measured mile, the emergency speed proposed will not seem unreasonable for a vessel of 3,000 tons displacement to those familiar with the law of speed in relation to dimensions as enunciated by the late Mr. Froude.

Nor would it be necessary to build vessels so large as 3,000 tons displacement in order to combine high speed with great defensive and offensive powers, for extremely useful vessels could be constructed on 1,500 or 2,000 tons displacement, and as a cruiser can only be in one place at a time, no matter how great her size, and as a small vessel upon the plan proposed would be as effective as a commerce destroyer as the largest, it would seem to be the best policy to devote the small appropriations obtainable for the increase of the navy to the construction of a greater number of small ships, rather than to a less number of large ones.

Two such small swift cruisers of 2,000 tons displacement, armed with 10½″ pivot guns, mounted on vertical V shields, and with a proper complement of Hotchkiss revolving cannon, having an emergency speed of 18 or 20 knots, and provided with means of discharging rocket torpedoes, would be more than a match for an *Inflexible* or an *Italia*, as, owing to their small size and rapidity of movement, they would be very difficult to hit, either with shot or torpedo, while their unwieldy adversary would fall an easy victim to the latter weapon discharged from the two cruisers. The cost of construction and maintenance of one vessel of the *Inflexible* or *Italia* class will be found to be four times greater than that of two of the proposed cruisers of 2,000 tons displacement, while the fighting strength of the two latter combined, will be more efficient than that of one of the former.

CAST IRON IN STEAM BOILERS.

By S. Lloyd Wiegand.

[Read at the Stated Meeting, Wednesday, March 19, 1884.]

Immediately after the adjournment of the February meeting of this Institute, the drum having flat cast iron heads, was tested by hydrostatic pressure, for the purpose of ascertaining the strength of such heads.

The dimensions of the drum were four feet in length by thirty-six inches in diameter, the heads were of cast iron, of 18,000 lbs. tensile strength per square inch of cross section and one and fifteen sixteenths inches ($1\frac{15}{16}$) thick; in one head was a man-hole opening of about 10 x 14 inches, below it a feed inlet of two inches diameter and having a flange fastened thereon by four $\frac{5}{8}$ stud bolts and nuts; there were three openings for guage cocks and two for a glass water guage connection, such as is usual in cylinder boilers having this form of head. These openings were closed with screw plugs, the man-hole opening by a plate not planed, but simply cast with a flat bearing surface, and a gasket of vulcanized India rubber, interposed between it and the slightly raised bearing on the inner surface of the head which had been planed flat.

The other head was simply a casting without any perforations therein excepting of course the rivet holes in the flange.

This drum or cylinder was made by the same parties, Messrs. Sidebotham & Powell, of Frankford, who made the boiler which exploded in Gafney & Nolen's dye works, on Martha Street above Huntingdon Street, Philadelphia, on May 25, 1881, and the heads were cast from the same pattern, and fitted as nearly as they could be in the same way as the burst boiler head, the remaining parts of which were produced at the February meeting.

The test was made with a force-pump, having a $\frac{5}{8}$ in. diameter ram and two pressure guages, each provided with a maximum registering hand, and graduated in 25 lbs. divisions.

The following results were shown: at 425 lbs. to the sq. in. the longitudinal seam leaked just enough to show a wet line, as did also a small part of the seam between the cast iron head containing the man-hole; at 550 lbs. the head cracked so as to make a noise, but showed very little leakage, the cracks being two radial ones from the man-hole opening, and one some 10 inches around the rim or flange, where it

joins the fillet uniting it with the flat part of the head; upon pressure being pumped up to 820 lbs. per square inch, about one-fifth of the head fell out, emptying the vessel of water in less than three seconds.

One of the guages used for this test was located on the highest part of the drum, and the other on the pipe close to the pump.

Up to the time of the breaking out of the piece, not more than a half pint of water escaped, although between 550 lbs. and 820 lbs. pressure, there were about thirty 6 inch strokes of a $\frac{5}{8}$ in. diameter ram, made in about $\frac{1}{4}$ of a minute.

The feed water inlet, which was exactly the same as in the Gafney boiler head remains intact, and the fracture is through no other opening than the man-hole.

The back head of the drum expanded very nearly $\frac{3}{16}$ (within ·005 in.) of an inch, in the experiment, and resumed its former shape and appears to be unimpaired in strength.

It had been stated, in the course of the discussion of a paper on this subject, that such heads were unsafe at any pressure, and under a formula submitted on the blackboard to the Institute by a member, the ultimate strength of the head without openings in it, should have been between 25 and 30 lbs. per square inch.

The result of the test shows that such formula was entirely at fault, and that the strength of such structures is far in excess of any recognized factor of safety for the service to which they are applied, and most strongly suggests that the alarm attempted to be raised upon the subject was without reasonable foundation.

The propriety of experimentally ascertaining the properties of such structures, and the rules which should govern their construction and proportion, as now being done by the Committee of Science and the Arts, is obvious.

New Method of Insulating Telegraphic Wires.—C. Widemann, having applied the processes of Nobili and Becquerel, for coloring by means of alkaline plumbates and ferrates, observed that the pieces thus colored resisted all galvanic action and their surfaces no longer conducted the electric current. A wire of copper or brass, or even of iron, may thus be covered with an insulating layer, analogous to that of a coat of resin or of gutta percha. The insulation is complete, easily accomplished, and at a very moderate cost. It is also very durable and but slightly affected by the different atmospheric influences.—*Comptes Rendus*, Oct. 15, 1884.

HANGING THE LEVERS FOR INDICATION.

By Robert Grimshaw.

Where levers are used, whether slotted or with dipping link, to reduce the crosshead motion of horizontal engines, they may be (1) swung from the ceiling, (2) inverted and fulcrumed at the floor, or (3) fastened to the wall and swung horizontally.

The first method is usually the most convenient, as being more out of the way of the engineer and the operator. Besides this, it permits of the use of a long pendulum, and thus gives minimum distortion of the card.

We usually find engine rooms with ceilings which permit of easy attachment of the pendulum. We may divide wooden ceilings into three classes, (1) those in which the joists run parallel with the center line of the engine, (2) those having the joists crosswise of the engine axis, and (3) those which are ceiled with boards or with lath and plaster.

Where the joists run lengthwise of the engine, it is very easy to hang a pendulum. An ordinary stout lag screw is used, and the pendulum put on that side of a joist which will bring the cord nearest in line with the indicator. By means of wooden washers between pendulum and joist and of projecting pins from the pendulum, the card may readily be "led fair," from the pendulum to the indicator. This is rendered easier by the fact that the indicator stem itself has a swivel

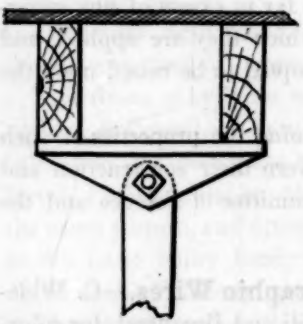


Fig. 1.

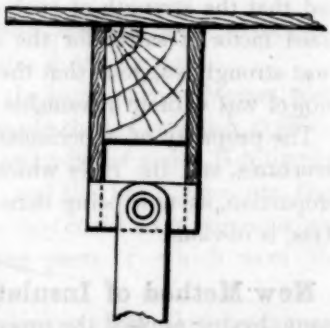


Fig. 2.

joint, giving it four or five inches of lateral adjustment when beside a horizontal cylinder, and more when on top.

Where the joists run crosswise of the engine, fasten across two of them, by lag screws, a wooden strip about $2'' \times 3''$, having another piece about $4'' \times 1''$ nailed to its edge; the latter piece having a $\frac{1}{2}''$ hole for the pendulum pivot. (Fig. 1.) Or, nail a strip three inches by one on each side of the joist, and nail across between these a bridge-piece with $\frac{1}{2}''$ hole. (Fig. 2.)

Where the ceiling is boarded, or lathed and plastered, fasten to it, by lag screws *or large gimlets*, the first angle piece recommended for crosswise joints.

Where there are sloping rafters instead of horizontal joists overhead, they generally run crosswise of the engine, and may have nailed across two of them a straddle-piece having its edge beveled to suit the slant of the roof.

There is one satisfaction about this case: it generally allows the use of a long pendulum, very easy to get in line with the instrument.

The straddle-piece may be either of scantling, as in Fig. 3, or of boards, as in Fig. 4.

Where ceiling attachment is impossible, as is sometimes the case by reason of steam pipes, etc., in the way, the inverted lever pivoted to

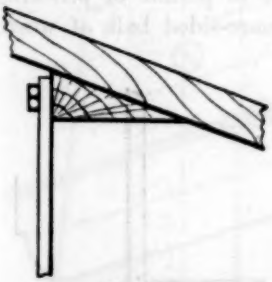


FIG. 3.

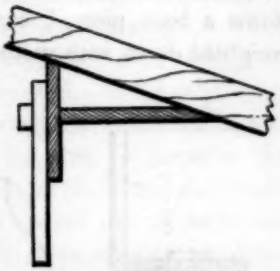


FIG. 4.

the floor is the handiest, if the floor happens to be of wood; but if of earth, cement, brick, stone, or iron, another method will have to be chosen.

But supposing the floor to be of wood, it is best to make a base piece, which can be fastened down snug against the bed plate of the engine, by screws or large gimlets. (Figs. 5 and 6.) A piece of $3'' \times 4''$

scantling 12 inches long, makes a good enough base piece, but care must be taken that the lever stands plumb.

An improvement in this gives lengthwise adjustment of the lever pivot, after the block is fastened down, so that the rig shall be in line with the crosshead attachment, if a slotted lever is used, or just the length of the dipping link in advance of it if the latter is employed. The pendulum, instead of being pivoted to the block directly, is attached to a right angle trough piece or ⌐, having in its upper side oblong bolt holes, to give it about an inch endwise adjustment. (Fig. 6.)

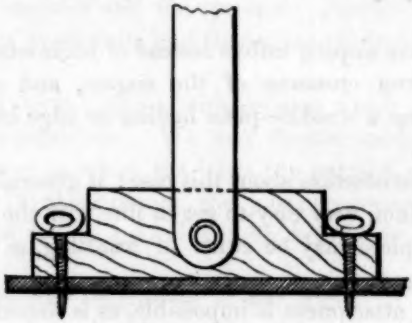


FIG. 5.

When the floor is of such a nature as not to permit of screwing down a base piece, I have used a heavy square-sided balk of wood, weighted down with castings. (Fig. 7.)

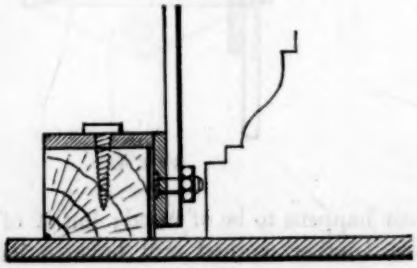


FIG. 6.

FIG. 7.

Wall attachments and horizontal pendulums are not at all to be recommended, as they are very much in the way, and the point of cord attachment on the pendulum is necessarily far out of the line of

the indicator. But they are on rare occasions necessary. It is generally necessary to make a bracket say $18'' \times 6'' \times 6''$ to nail or screw to the wall at the proper height for the crosshead attachment.

When this rig is used, special care must be taken to have the pendulum pivot, the cord attachment and the indicator pulley form a right angle when the crosshead is at midstroke; and when the slotted lever is used, to have the pendulum center exactly abreast of the crosshead attachment, when the latter is at midstroke.

If these precautions are not taken, the cards will be false; making cut off appear too early on one end, and too late on the other, but in different amounts.

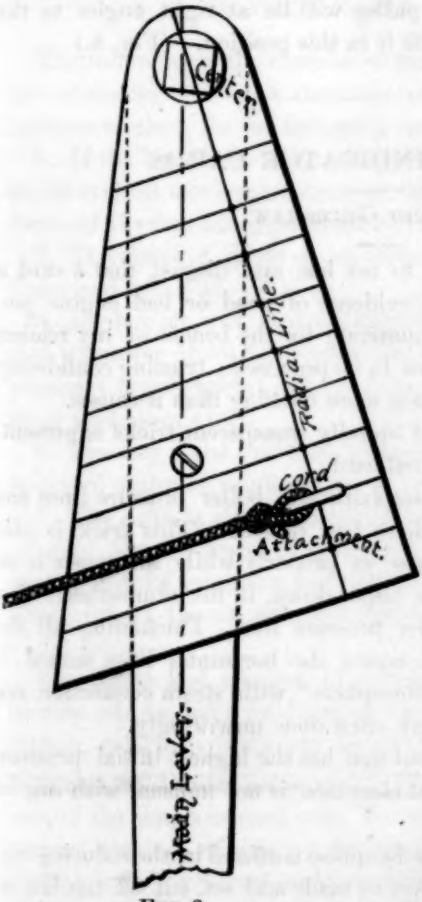


FIG. 8.

I do not know that I can say enough against the use of guide pulleys to bring the cord from the reducing lever to the indicator pulley.

There are very few cases where guide pulleys are at all called for. Perhaps, where overhead piping interferes, they may occasionally be necessary; but seldom elsewhere. They are troublesome to affix, are seldom stiff, and the cord is apt to ride them when slack, and get jammed in between the wheel and its frame, and break when hauled on. I have had to stop an engine that was driving two mills, in order to climb up on it and unhitch or replace the cords from this cause.

Unless there is something in the direct line between the cord attachment and the indicator pulley, guide pulleys are not necessary; as in order to get correct motion it is only essential to see

that when the crosshead is at midstroke, the cord attachment forms a right angle with the pendulum centre and the indicator pulley.

A good way to insure that the pendulum centre, the cord attachment and the indicator pulley form a right angle when the crosshead is at midstroke, is to have a separate arm in the shape of a right angled triangle, having scribed along near one leg a line containing the pendulum centre and cord attachment and having a number of parallel lines drawn at right angles thereto. This arm is pivoted on the same bolt which carries the main lever, and is adjusted to such an angle that the cord going to the indicator pulley will lie at right angles to this radial line. A light screw holds it in this position. (Fig. 8.)

DOCTORING INDICATOR CARDS.

By Robert Grimshaw.

I found out some years ago, to my loss and disgust, that a card is not by any means *prima facie* evidence of good or bad engine performance; and I shall here enumerate for the benefit of my readers a few of the tricks which I know to be practised; trusting confidently that this *exposé* will prevent much more tricking than it causes.

One of the most common and literally transparent tricks is presenting tracings instead of the original card.

Another is omitting the atmospheric and boiler pressure lines and drawing them in afterwards where best desired. This trick is also modified by drawing one of these in properly while the paper is on the barrel, and then shoving the paper down, if the atmospheric line is drawn, or up if it is the boiler pressure line. Thickening all the lines so as to bring the diagram nearer the horizontal lines named, is practiced; as is "taking the atmosphere" with steam connection not quite shut off. The latter is very often done unwittingly.

Taking cards only from the end that has the highest initial pressure or the freest exhaust, or the least clearance, is not unusual with one or two engine-builders.

About the best opportunity for deception is offered by the reducing rig.

We will suppose that the valves as made and set, cut off too late at the back end. The instrument is applied to the back end, and the cut-off point in the diagram set back from its proper place, if a reducing

wheel is used, by leading the cord at an angle from the true parallel with the piston rod, which it should occupy at all positions of the crosshead. If a lever rig is used, the cord angle is made such as to give, for the first quarter or eighth of the outstroke, too little motion of the paper drum; and for the expansion period too much.

THE SUN-EARTH BALANCE.

By Pliny Earle Chase, LL.D.

The following is the shortest method which has ever been published for estimating the sun's distance. I believe that it is also the most accurate method, for the following reasons.

1. If the hypothesis of an all-pervading luminiferous æther is true, all its cyclical movements must be rhythmic, or harmonic, the various forms of rhythm being governed by various centres of oscillation.

2. The simplest kinds of oscillatory motion, in cosmical bodies, are linear and spherical.

3. Laplace showed, in discussing the motions of Jupiter's satellites, that whenever there are tendencies to simple numerical relations, in planetary arrangements, all the forces of the system combine to make those tendencies exact.

4. The sun is the principal centre of attraction, and the earth is the principal centre of condensation in the solar system.

5. The earth is also a centre of linear oscillation for a point in the orbit of Mars, a centre of rotary inertia for a superficial film of condensation or of luminous undulation in the same orbit, and a centre of rotary inertia for an æthereal sphere which has its limit in the asteroidal belt.

The action and reaction of æthereal waves, between these two important centres, have produced an amount of gravitation, at the earth's surface, which is sufficient to give a circular orbital velocity of $\sqrt{gr} = 4{\cdot}9073$ miles per second. The linear oscillation of the earth around the sun, combined with the superficial resistance at the outer limit of the belt of greatest condensation, multiplies the energy by $\frac{3}{2}$, and the rotary æthereal oscillation multiplies it by $\frac{5}{2}$. Consequently, the earth's velocity of revolution, which measures the projection of *vis viva* against uniform resistance, is $\frac{5}{2} \times \frac{3}{2} \times 4{\cdot}9073 = 18{\cdot}4024$ miles

per second. There are 31,558,149 seconds in a year; therefore the sun's distance is $18{\cdot}4024 \times 31{,}558{,}149 \div 2\pi = 92{,}428{,}300$ miles.

The sun can be weighed by its musical rhythm, with a corresponding facility. Orbital velocities vary inversely as the square root of the distance from the centre of gravity. Any two attracting masses bear the same ratio to each other as the distances at which they would communicate equal orbital velocities, to particles which revolve about the centre of gravity of the attracting bodies. Hence we have

$$\overset{\text{Earth's rad. vec.}}{92428300} \times \left(\tfrac{5}{2} \times \tfrac{3}{2}\right)^2 : \overset{\text{Earth's rad.}}{3962.8} :: 327994 : 1.$$

In other words, the sun weighs 327994 times as much as the earth.

There are many other harmonic tendencies, which are introduced by the masses and distances of other planets, as well as by the correlations of electrical, chemical, and other forces, which may slightly modify these estimates. There is, however, no likelihood that the adjustments of universal equilibrium would make a secular variation of $\frac{1}{2}$ per cent. in the earth's mean distance from the sun, and there has never yet been any astronomical estimate of that distance which has so small a probability of error as the one here given.

Injurious Effects of Illuminating Gas.—Dr. Arnozan recounts many accidents, which have fallen under his observation, among persons who have used gas for cooking. These accidents, which are almost always of a medical character, are especially frequent when the gas is burned in portable furnaces, which are not provided with a suitable chimney for carrying off the products of combustion, or when the gas is allowed to remain in rubber tubes. The latter risk may be mostly avoided by shutting the gas off from the tubes whenever the apparatus is not in use; but even then the rubber wears away so rapidly as to require frequent replacement. Whenever gas is used for cooking it should always burn with a blue flame, as in the Bunsen burner, on account of neatness, economy and health. Unfortunately the combustion of the gas is usually very incomplete, on account of the deterioration, or the bad arrangement, of the apparatus. Generally, also, kitchens are badly ventilated. Some accidents are reported from burns occasioned by the explosion of a detonating mixture at the moment when the gas is turned on.—*Génie Civil.; Les Mondes*, Feb. 2, 1884.

STANDARDS OF LENGTH AND THEIR SUBDIVISION.

By GEORGE M. BOND, Hartford, Conn.

[A lecture delivered before the FRANKLIN INSTITUTE, February 21, 1884.]

(Concluded from page 295.)

After having thus briefly considered the subject of the "evolution" of a standard, and the conditions under which it must continue in order to be worthy of being called a standard, we will now attempt to show some of the methods adopted for comparing these yard or meter bars, and explain some of the principles upon which the accuracy of the comparison depends.

We have already partly described the way in which the end meter is compared or transferred to a line measure by the reflection of a fine point of platinum, without actually touching the ends of the standard bar. We may now notice how two standard end measure bars may be compared, using a method by which the differences, if any, are greatly magnified, and are thus very readily determined.

A most ingenious application of the laws of the reflection of light was made by Joseph Saxton for comparison of end-measure bars, and for which, in recognition of its value to science, he was, in 1837, awarded the John Scott Legacy Medal, his invention being the Reflecting Comparator. It depends upon the magnified distance of the path of a reflected ray of light, caused by the rotation of a mirror placed vertically, and delicately pivoted, the spindle of the mirror being connected with a sliding bar by a fine watch fusee chain wound around the barrel of the mirror spindle. At the end of the sliding bar, to which this chain is attached, contact is made with the end of the standard to be compared, the other end of the standard being firmly abutted against an immovable "stop."

By first placing the standard bar in position, care being taken to have the bar supported, as you will remember, at the "neutral points," and exactly in line, so that the centers of the opposite ends of the standard are against the contact surfaces of both the stationary and the sliding stops—and which, by the way, is one of the most difficult features of the experiment—a ray of light is brought to bear upon the mirror, and the reflection of a circular scale is observed through a small telescope, mounted just above this divided arc. This circular scale may be placed at any convenient distance from the mirror, say 15 or 20 feet.

It is evident that a very slight motion of the sliding bar, *G*, in the figure shown upon the screen, (Fig. 1), will cause a ray of light, reflected from the mirror, *M*, to which its motion is imparted through the small chain and drum, to move with a much greater velocity at the distance of the large circular scale, *R S*, and, as the angle of incidence is equal to the angle of reflection, a motion of the mirror through an arc of 5 degrees would cause a motion of the reflected ray of 10 degrees, as we may readily understand by taking the geometrical proof in illustration.

A polished surface is placed so that the light strikes it "squarely,"

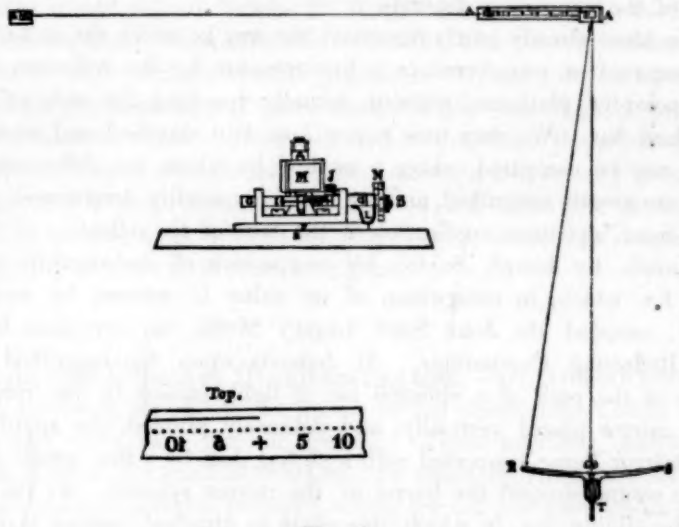


FIG. 1.

or, in other words, at no angle whatever; it will evidently be reflected directly back to its source. Now, suppose it is rotated into such a position as indicated in the accompanying figure, (Fig. 1*a*), which is just 45 degrees as compared with its original position, the light still coming from the same direction; it now strikes it at an angle of 45 degrees, and as light is always reflected at the same angle as that at which it strikes a polished surface, its new path will be again 45 degrees from the plane of the mirror; but, as you will see, it is twice 45 degrees with respect to its incident path, and is thus reflected at an angle of 90 degrees.

We can readily see how extremely delicate or sensitive to the slightest change of position this reflected ray becomes. As light may be said to have no weight, and consequently no momentum or inertia, it will quickly and certainly indicate the slightest change in length of a standard end measure bar.

By calculating the length of the relative "lever arms" we can

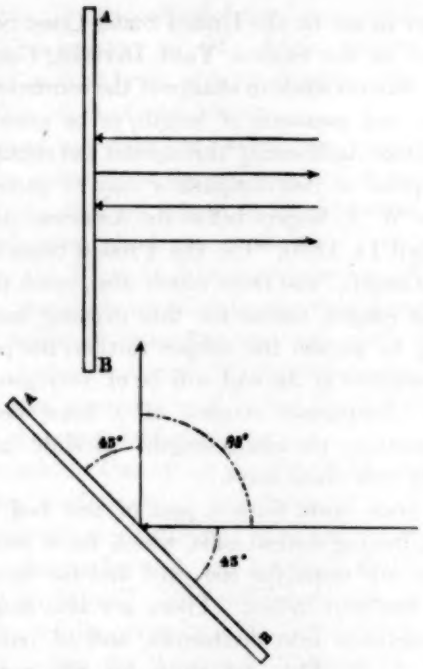


FIG. 1*a*.

easily determine the magnifying capacity of such an instrument of precision. For instance, supposing the drum on the spindle to which the rotating mirror is attached is one-quarter of an inch in diameter, and that the length of the radius of the large circular scale is 20 feet, we have, using the double angle in this relation, the distance moved by the sliding bar touching the standard, as compared with the arc passed over by the reflected ray at the distance of 20 feet from the mirror, and reducing to the same unit as $\frac{1}{8} \times \frac{1}{12} \times \frac{1}{40} = \frac{1}{3840}$ is to 1, or as 1 is to 3840, hence a motion, or variation of one-thousandth of an inch at the point of contact would be 3·84 inches at the scale.

By placing a metallic bar in a closed tube, the ends merely projecting through this tube, and filling the tube with ice water, and then with water of a known higher temperature, and comparing the lengths of the same bar under these varying conditions, the amount of expansion for each degree can be determined; this will give us what is called the coefficient of expansion, to which reference has already been made.

The comparator in use by the United States Coast Survey at Washington, designated as the Saxton Yard Dividing Comparator, is one designed by Mr. Saxton while in charge of the construction of standard balances, weights, and measures of length, to be presented to the different States, to insure uniformity throughout the country.

A short description of this comparator may be quoted from a paper read by Professor W. A. Rogers before the American Academy of Arts and Sciences, April 14, 1880, "On the Present State of the Question of Standards of Length," and from which, also, much that is of interest in regard to our subject matter for this evening has been obtained. Any one wishing to pursue the subject further, the paper entire, and the references contained at the end will be of very great assistance.

"The Saxton Comparator consists of a brass bed-plate, having V-shaped ways running the entire length. A slide carrying a microscope slides freely over these ways.

"A series of brass posts form a part of this bed, through which pass steel screws, having conical ends, which have been tempered and polished. There are stops for the yard and for its subdivision into feet, and of one foot into inches. There are also stops for the meter and for its subdivision into decimeters, and of one decimeter into centimeters. * * * The end stops for the yard and for the meter were, many years ago, set to correspond with "Bronze No. 11," at 58° nearly for the yard, and with the iron meter at 68° nearly. * * * The standards which have been distributed since 1856 have been transferred from these distances at the temperatures at which they are standard.

"The yard in actual use at the Bureau of Weights and Measures, therefore, may be defined to be the distance between two steel stops attached to the bed of the Saxton Comparator which corresponds to the length of Bronze No. 11, at 58° nearly, and the meter may be defined to be the distance between two steel stops of the Saxton Comparator which corresponds to the length of the iron meter corrected for the

difference between its length at 32° and at 68°, nearly. Recent comparisons indicate that these temperatures should be diminished, by a trifling amount, for the present distances between the stops both for the yard and for the meter."

Engravings representing the Saxton Yard Dividing Comparator and also the Saxton Reflecting Comparator here shown, were obtained through the kindness of Prof. J. E. Hilgard, Chief U. S. Coast Survey, by whom every facility was afforded me for examining the methods of comparison. The courtesy of Mr. Blair, assistant in charge, has aided me greatly in thus being able to illustrate the instruments now in use at the office of the Coast Survey.

Another form of a comparator, which has proved to be successful in the use of the means "for the end sought," in the comparison and investigation of standards of length, is that known as the Rogers-Bond Universal Comparator, which was constructed from plans proposed by Prof. Rogers by the Pratt & Whitney Company, of Hartford, Conn., for their use in practically establishing standard gauge dimensions. A duplicate comparator of this form was also made by them for Prof. Rogers for his professional work at Cambridge, and for the transfers and comparisons of standards used by the Pratt & Whitney Company as the basis of these standard sizes.

The comparator at Cambridge is also used by Prof. Rogers in determining the coefficients of expansion of the various materials used in the construction of standard yard and meter bars, and also for obtaining the relation between the length of the Imperial Yard and the "Mètre des Archives." The solution of this latter interesting and difficult problem is fully given in a Memoir by Prof. Rogers, presented May 9th, 1883, before the American Academy of Arts and Sciences, entitled "Studies in Metrology," and to which reference may be had.

The special features of the Universal Comparator are, as its name implies, the variety of the methods employed and the range of work that can be done in comparing standards; each independent method, when carefully carried out, producing similar results which serve to check or prove the comparisons. It includes a method for investigating the subdivisions of the standard by comparing each part of the total length with a constant or invariable quantity or distance.

By the aid of the diagram of the plan and elevation of this form of comparator, the aim being to exhibit *principles* rather than a picture

of the instrument, we may be able to describe in a few words the main features of its construction. (Fig. 2).

A heavy cast iron base *A*, is mounted upon stone capped brick piers, giving a permanent foundation to the apparatus. Upon this base, and reaching from end to end, are two heavy steel tubes, *B* and *C*, three inches in diameter, ground perfectly straight, and being "true" when placed in the centres of a lathe, the object being to get a straight line motion of the microscope plate *D*, which slides freely on these true cylinders.

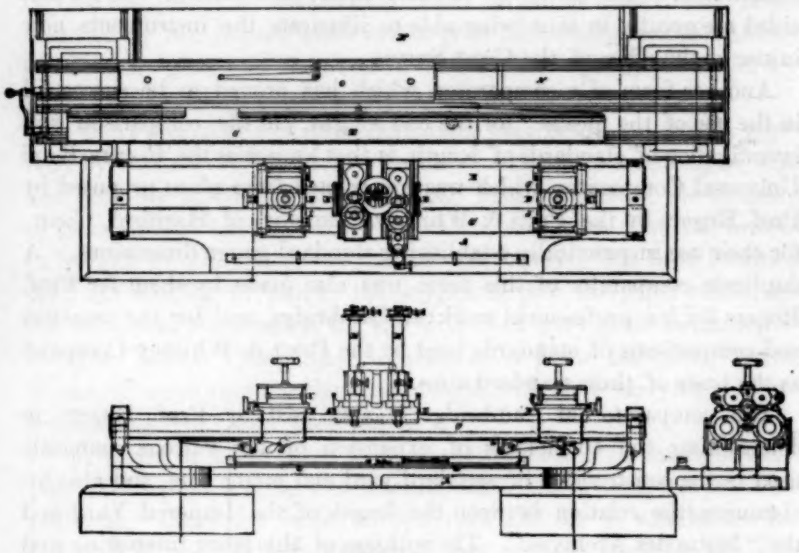


FIG. 2.

Flexure of these cylindrical guides is provided for, by lever supports at the neutral points *n* and *n*'. Fitted closely to these guides, and outside of the range of motion of the microscope plate *D* are two stops, *E* and *F*, one at each end, as shown in the figure. These stops are arranged to be adjusted at any desired position along the guides, and are securely held by clamping on the under side by the handles *G* and *H*.

These stops are each provided with a pair of electro-magnets, *I* and *J*, the poles of which do not come in contact with the armature seen at either end of the microscope plate. Contact is made at *K* and *L*, which are hardened steel surfaces, tempered and polished, and placed as nearly as possible in the centre of the plate and of the stops.

The magnets are intended to overcome the unequal pressure due to ordinary contact, a rack and pinion being used to move the plate. The magnets are used to *lock* the microscope plate at each end of its traverse between the stops. The use made of this sliding microscope plate and the stops we shall see presently.

Beyond the main base just described, and supported also on brick piers, is an auxiliary cast iron frame *N*, which is provided with lateral and vertical motion within limits of zero and 8 and 10 inches, respectively, for rough or approximate adjustment, and upon the top of this frame are two carriages, *O* and O^1, which slide from end to end, a distance of about 40 inches. Upon these sliding carriages are placed tables *T* and T^1, provided with means for minute adjustment, for motion lengthwise, sidewise, and for leveling, thus permitting the adjustment of a standard yard bar quickly, and without the necessity of its being touched with the hands after being placed upon the table until the work of comparison is completed.

Before describing the operations necessary for a series of comparisons, it may be well to explain the peculiar fitness, for purposes of this kind, of the microscopes *M* and M^1 used in this connection.

The tubes are 12 inches long and 1¼ inches diameter, the eye-piece micrometers m_1 and m_2 were made by Joseph Zentmayer, of this city, whose skill as an optician is too well known to require further proof of their excellence. The objectives were made by the late Mr. R. B. Tolles, of Boston, and are each fitted with his illuminating prism.

In order to use a microscope upon lines ruled on polished surfaces, or on any opaque material, some means for obtaining sufficient light must be employed to see them distinctly, without the use of reflectors, which are often a source of error in standard work.

In no other form of objective does this requirement seem better fulfilled than in that invented and made by Mr. Tolles. The objectives are each fitted with a prism of perfectly clear glass, placed just above the lower lens, and one end of the prism passes through the side of the objective. The inner end of this prism is beveled, (Fig. 3), forming such an angle of the end surface to the axis of the prism, that light is refracted perpendicularly upon the surface of the bar, lines less than $\frac{1}{80000}$ of an inch in width being easily seen and separated with a one-inch objective. It may be said to "carry its own lantern," and with light so thrown, just where it is most needed, the *bottom* of the cut or furrow of a line cut by a diamond edge, as fine as that just

stated ($\frac{1}{30000}$ of an inch), as well as the *edges* of the furrow, can readily be seen.

This method of illumination has proved to be *invaluable* in the work of comparing line measure standards, especially so in the case of bars having the lines ruled on polished gold surfaces at the bottom of wells sunk one-half the depth of the bar, these wells being not over one-half an inch in diameter, as in the case of Bronze 1, and also of the bar now before you.

The first operation in the use of this form of comparator is to level the main base, *A*, then sliding the microscope plate *D*, from end to end of the steel tubular guides, having the microscope adjusted so as to be in focus upon the surface of mercury held in a shallow trough, over which the microscope passes, the curvature due to flexure of the guides

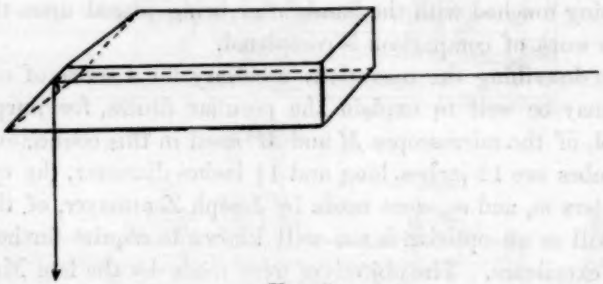


FIG. 3.

is determined, and may be compensated for by counter weights at the neutral points of support, n and n^1.

In order to test this right line path of the microscope plate horizontally, the method of the "stops" is employed, or, another method, which is that of tracing a fine line the entire length of a standard bar upon its upper surface, and reversing the bar, tracing another line very near the first and at an equal distance apart at each end; then if this distance is uniform between the two lines the entire length, it is safe to assume that the path of the plate is a straight line horizontally, and at the middle, the amount of curvature, if any, and also if regular, is readily determined. This method has been used by Professor Rogers with marked success.

The "stop method" is to compare a line measure, or an end measure bar, on each side of the centre line of motion of the microscope plate, using *one* microscope, and comparing this fixed length with the con-

stant quantity before referred to, which is the distance between the stops. Should the path be a curved one, the distance between the defining lines upon the bar will appear greater on one side than on the other in proportion to the amount of curvature existing. The length of the standard being the chords of circles of different radii, but by comparison with the stops, seems really to be different in length at each position, caused by the different distance, through a larger arc passed over by the microscope. By means of the proportion of similar triangles, the lengths of the radii may be very accurately determined.

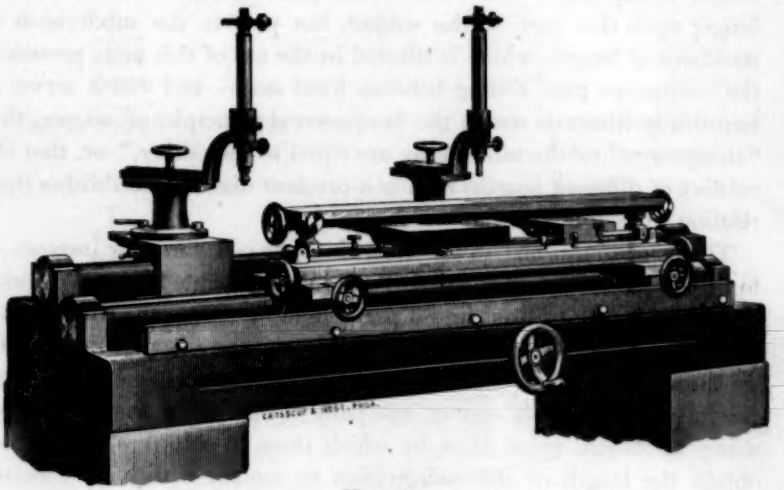


FIG. 4.

By placing different standards on one side of the line of the stops, they may be, by being compared with a constant quantity, compared also with each other.

Another method for comparing two or more standards, is to place two microscopes one on each of two microscope plates upon the guides, at a distance determined by the length of one of the standards, and by replacing this one by a second, the coincidence of the lines in the eye-piece micrometer, or their variation, showing their relation. The microscopes may be placed horizontally in this same fixed relation, using the method invented by Lane, and which has been used in the office of the U. S. Coast Survey at Washington.

A modification of this form of comparator, (Fig. 4) made by the Ballou Manufacturing Company, of Hartford, Conn., for Prof. Anthony, of

Cornell University, is here shown. The instrument is mounted upon a single heavy base. Though not having the range of motion of the adjustable support for standard bars shown in front, as is possible with the original comparator, it possesses all of the conveniences for rapid adjustment and accuracy of movement. The right line motion of all moving parts longitudinally, is governed by heavy cylindrical guides, and the same method of the "stops" is used in investigating the subdivisions of a standard bar.

There are five independent methods for comparing standards of length by the use of this form of Comparator, but we will not dwell longer upon this part of the subject, but pass to the subdivision of standards of length, which is effected by the use of this same process—the microscope plate sliding between fixed stops—and which serves to beautifully illustrate one of the fundamental principles of science, that "things equal to the same thing are equal to each other," or, that the *relation* of different lengths each to a constant distance, establishes their relation to each other.

This is accomplished in the following way: A yard, for instance, is to be subdivided into 3 equal parts, or into 3 separate feet. We divide the whole length by trial into 3 parts, then by setting the stops so that the microscope plate may move very nearly the distance represented by the first one of the 3 parts, by readings of the eye-piece micrometer carefully taken at each end of the path of motion of the microscope, using the finely ruled lines by which these 3 parts are defined, we obtain the length of this subdivision as compared with our constant quantity; then by sliding or moving the bar along under the microscope until the second part is in place, the same operation is again performed, and so for the third, thus determining the relation for each with this arbitrary or temporary standard; then by adding the differences between these separate parts and the constant length, and taking the mean or average of these differences, from which we subtract each difference, gives us the correction to be applied to each part in order that it shall be *exactly* one-third the total length, or, as in case of a yard bar, giving us exactly 12 inches or a standard foot. The foot may then be subdivided in the same manner into 12 equal parts, establishing a standard inch, and further to eighths, sixteenths, thirty-seconds, hundredths, or thousandths of an inch.

To illustrate this method, and to make plain the reason why these corrections so obtained are used, we can suppose a case of simply dividing a rod or a string in two parts. Now we know that for what-

ever amount one part is longer than the other, one half of this amount belongs to the shorter to make it exactly one-half the whole length of the rod or string; hence we have one-half the sum of the difference, and subtracting each difference from this half sum, would in one case, give us a *minus* correction for the longer part, and a *plus* correction to be applied to the shorter.

A series of readings or "observations" using the microscope with the eye-piece micrometer, and having the subdivision of a standard yard into three equal parts, to determine, would be after this form:

First Foot.

L.	R.
3·68·5	3·98·2
3·68·7	3·97·8
3·68·3	3·98·5
Mean 3·68·5	Mean 3·98·2
	R − L = + 29·7

Second Foot.

L.	R.
3·57·4	3·87·3
3·57·5	3·87·7
3·57·9	3·86·9
Mean 3·57·6	Mean 3·87·3
	R − L = + 29·7

Third Foot.

L.	R.
3·61·3	3·97·0
3·62·0	3·97·8
3·61·2	3·97·7
Mean 3·61·5	Mean 3·97·5
	R − L = + 36·0

Correction.	Σ
+ 29·7	+ 2·1
+ 29·7	+ 2·1 + 4·2
+ 36·0	− 4·2 ± 0·0
3)95·4	
Mean 31·8	

The column under "L" being readings taken at the left or initial end of each foot, and "R," readings taken at the right, "R — L" being the difference between the readings taken at each end of this subdivision of the whole length.

The column under "correction" shows the amount in divisions of the micrometer needed to make each foot exactly one-third the yard. Under "Σ" these corrections are added as a check upon the accuracy of the work in case of a long column of corrections, as when the foot is subdivided into inches, or an inch into 16ths or 32nds.

We have thus traced, briefly, the development of the standards of length from some of their rudest units to that of the present British Imperial Yard and its copies, and the metre, and shown how the yard has in one way at least, been subdivided within a limit of about one hundred thousandth of an inch, it remains now to show in what way these accurate subdivisions may be successfully applied to every day use for work requiring such nicety, and in our next lecture it is hoped that our efforts may not prove unsuccessful.

Hartford, Conn., January 11, 1884.

STANDARDS OF LENGTH AS APPLIED TO GAUGE DIMENSIONS.

By GEORGE M. BOND.

[A lecture delivered before the FRANKLIN INSTITUTE, February 29, 1884.]

In our lecture of last week we attempted to show in what way Standards of Length may be constructed, and how these standards so constructed may be subdivided within very close limits of error. We will now attempt to show in what way these subdivisions may be applied to the requirements of modern shop practice, and how the gauges or implements used in work requiring interchangeability of parts are made so as to insure this accuracy.

Manufactured articles have been compared by Sir John Herschell to atoms, on account of their uniformity. The uniformity of manufactured articles may be traced to very different motives on the part of the manufacturer. In certain cases it is less troublesome as well as less expensive to make a great many articles exactly alike, than to adapt each to its special requirements. Thus, shoes and the uniforms for soldiers are made in large numbers, without any design of adaptation to the requirements of any one man. In another class of work the uniformity is intentional and is designed to make the manufactured articles more valuable owing to this uniformity. Thus, for instance, bolts and nuts of any particular size, if alike or interchangeable, may be replaced when worn out or lost, saving an immense amount of trouble, and especially valuable time, in cases of repairs where the stoppage of machinery or the delay of a train of cars would be a matter of serious loss, not only of time but also of dollars and cents.

In the third class, not a part only, but the whole of the value of the object arises from its exact conformity to a given standard. Weights and measures belong to this class, and the existence of well-adjusted standards of weight and measure in any country furnishes the evidence of the existence of a system of law that regulates the business of the people, enjoining in all measures a conformity to the national standard.

There are thus three kinds of usefulness in manufactured articles: cheapness, serviceableness, and quantitative accuracy. Which of these was referred to by Sir John Herschel, we cannot say. It is as likely the last as the first, though it would seem more probable that he meant to assert that a number of exactly similar things cannot be, each of

them, eternal and self-existent, and must therefore have been made. Hence he used the phrase "manufactured articles" to suggest the idea of their being made in great numbers (*Encycl. Brit.*, 9th edition, vol. 3, p. 49).

Adam Smith, the founder in England, of the science of political economy, in his most important work, "The Inquiry into the Nature and Cause of the Wealth of Nations," referred particularly to the benefits derived from a systematic division of labor. He showed by apt illustrations the wonderful results to be attained by this now well-known principle, both as regards the quality and the quantity of the product.

During the score of years from 1765 to 1785, when Adam Smith was working out his memorable treatise just referred to, the inventions which have given us the steam engine and the loom were being perfected. While Adam Smith was lecturing in Glasgow, from the chair of moral philosophy, James Watt was selling mathematical instruments in an obscure shop within the precincts of the same university, and was working out *his* inquiry into the practical methods of applying steam.

In a paper read before the Institution of Mechanical Engineers, at Birmingham, Mr. Edward A. Cooper states that in a letter written to a friend, Watt thought he had attained remarkable mechanical accuracy when a cylinder he bored was so true that he could not get half-a crown between the piston and the cylinder, anywhere!

We must not be surprised at this remark when we consider the materials he used in making his models, and the probable state of the art of making machinery interchangeable at the time he lived. He used tin cylinders, and soldered the joints in many instances. Often he found it gave better results to *hammer* them rather than bore them. A block tin cylinder 18 inches in diameter, $\frac{1}{4}$ inch thick, when bored was found to be $\frac{3}{8}$ of an inch out of truth. He speaks of hammering it with a mallet outside, using a round piece of wood to correct this defect.

Eli Whitney was the first to develop the principle of quantitative accuracy by the use of the system of interchangeable parts in the manufacture of arms for the United States Government, early in the present century. As an evidence of the value of this system, in the year 1822, Mr. Calhoun, then Secretary of War of the United States, admitted to Mr. Whitney that the Government was saving $25,000 per year at the

two public armories alone, by the use of his improvements. This admission, the figures being probably far below the true facts of the case, serves to show that Mr. Whitney deserved well of his country in this department of her service. Mr. Whitney was noted for his exactness, his motto being, that "there is nothing worth the doing that is not worth doing well" (*Am. Journal of Sciences and Arts*, vol. 21, Jan., 1832).

In a paper read by Mr. Chanute before the American Society of Civil Engineers, at a meeting held in Washington, June 21, 1882, on "Uniformity in Railway Rolling Stock," he stated that the average cost of repairs in the shops of the New York, Lake Erie and Western Railroad, for the five years prior to 1875, was 9·17 cents per mile run by locomotives, while for the past five years it was only 4·33 cents. This represents a saving of about $675,000 a year. This was after the system had been adopted by the railroad company of making parts of locomotives in duplicate, using gauges and templates for this purpose. Had the rate of cost of 1871 prevailed in 1881, the expenses of locomotive maintenance would have been $790,492 greater than they were. The conclusion must not be formed, however, that all the above savings, or even a major part of them, have resulted alone from the system above mentioned. Much of the economy is doubtless due to other reforms introduced by the management of the road about the same time; but a considerable part is certainly due to the adoption of rigid standards and of interchangeable parts. Moreover, a very considerable number of the old engines still remain with all their imperfections, so that further benefits may be expected to result from the system as it becomes extended in the future.

In the system upon which the gauges produced by the Pratt and Whitney Company are based, the sizes are all constructed from accurate subdivisions of the British yard, made so carefully that any subdivision of a foot, taking any sizes from a quarter of an inch to four inches, varying by sixteenths, the sum or combination of these sizes taken at random and in numbers, or in sufficient lengths to constitute a foot, will be found to produce in the total sum, exactly the same result.

When we consider that in the experiment just mentioned, the variation of only one-thirty thousandth of an inch in each, if all one way, either plus or minus, would amount to an error in some cases of over half a thousandth of an inch, and particularly in the case of one combination where

fifteen or sixteen sizes were added, it will be seen that the error would be very perceptible in the test which they would thus undergo.

This severe practical test was applied by the Committee on Gauges, of the Society of Mechanical Engineers, in their investigation of this system of making standard gauges, these end measure pieces being found to be within the limit of accuracy necessary to fulfill this condition.*

In the production of these end measures, it is necessary that the end surfaces be perfectly parallel. This is a matter which is a simple operation as done by the Pratt and Whitney Company. Two sides of an end measure standard, such as the one we have now before us (Fig. 1), are made as nearly perfect planes as is possible, and at right angles to each other. The ends are then made perpendicular to these two surfaces, by means of a simple fixture which holds the end measure vertically and clamped in the angle of a movable block of cast iron which slides

FIG. 1.

freely over the plane surface of another block also made of cast iron. In the centre of this latter block is a copper matrix having diamond dust or washed emery in its upper surface. The end measure is passed rapidly over this surface, and being held perpendicularly, its highest points are ground away, and eventually this surface becomes a polished plane. The bar is reversed and the same conditions are applied to the other end; both ends being perpendicular to the same planes, are consequently parallel to each other.

These parallel surfaces being true planes, are, when brought together, capable of sustaining the weight of either one or the other, or in the case of the two which we have before us they may be held horizontally and still not separate. In a lecture at the Royal Institution, June 4th, 1875, Dr. Tyndall states that experiments by Robert Boyle, with plane surfaces placed in contact, show this clinging tendency even in a vacuum; and that with the surface plates he used, made by Whitworth,

* See Trans. Am. Society Mechanical Engineers, 1882, Vol. iv, report of Committee on Gauges, page 26.

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the force necessary to pull them apart was thirty times greater than that due to gravity, showing a mutual attraction or actual cohesion of the two surfaces. This is evidently the case, for we know that if the particles or atoms of a piece of steel are closely enough associated, they form a solid mass. By making their condition, artificially, as nearly like this as possible, the atoms are brought comparatively near each other, and more or less of this cohesive force results.

Of course, in the present case we have, apparently, the weight of the atmosphere to produce this result, but if we consider how small the surface is on which this weight is acting, we must admit that part of this clinging tendency must result from a cohesive force, as in the case before us the surfaces of these end measure pieces are less than a quarter of an inch square, and the weight of air, if there was a perfect vacuum between the two surfaces, would be scarcely enough to sustain this weight, were the surfaces perfect planes.

Perhaps the most marked example of interchangeable work resulting from a standard gauge system is that shown in the thread gauges which represent the Sellers or Franklin Institute thread. This form, proposed by Mr. William Sellers, on account of being adopted by the Government has been called the United States standard thread. In order to produce these standard gauges and to be able to guarantee them as being standard, it was necessary first to establish a standard inch. This standard inch must be one thirty-sixth part of the British Imperial yard, no more, no less. Then having obtained this standard inch, the subdivisions of it were to be obtained. So much for the size. Then in order to produce sixty degrees for the angle of the thread, it was necessary to establish this in a practical way, in order to furnish a gauge by which tools could be made that would insure absolute practical accuracy. This master triangle, designed by Mr. J. W. Heyer, furnishes the means of originating a triangle which shall be equi-angular, and consequently possessing angles of sixty degrees.

In order to obtain accurately the width of the flat, which is one-eighth of the pitch, for top and bottom of the United States Standard thread, it was necessary to establish a model triangle as a starting point, in connection with this master triangle. This model was, when finished, two inches long on each side.

The method used for obtaining a triangle having sides known to be exactly two inches long, without the necessity of their being actually measured, is as follows:

An eight-inch master triangle was so constructed that its center was definitely located by making it of parallel pieces of steel, accurately scraped, and fitted together. Within this triangle, its inner sides tangent to the circumference, was fitted a cylindrical plug or centre. This plug having been turned upon a true mandrel, the condition of the hole passing through it exactly in the centre, naturally took care of itself. A second cylindrical plug, hardened and ground, was next fitted to the centre plug. This hardened cylinder was ground to size exactly equal to the diameter of a circle which should, theoretically, be tangent to an equilateral triangle, whose sides are two inches long. Its diameter is $\frac{2}{3}\sqrt{3}$ or 1·1547 inches.

By holding securely a triangular piece of hardened steel upon the stud passing through the center plug, and having its faces or sides reversed in position as regards the sides of the large triangle, the sides are ground parallel to the sides of the master triangle, and also ground until a sharp edged corrected square, held against the side of the large triangle, determines the tangency of the sides to the inscribed circle 1·1547 inches in diameter.

As the large triangle is carefully tested for equality of angles, this inscribed circle furnishes the remaining data for producing a triangle whose sides are known to be two inches long, without their being measured.

In fact it would be impossible to measure them in any other known way within the limit this method makes entirely practicable, and which at the same time "fortifies" each step in the process by employing fundamental principles, and keeping the limit of error within what is claimed, which is $\frac{1}{10000}$ of and inch. Imagine anyone measuring the sides of a two inch triangle by contact with the almost infinitely sharp edges where the sides do meet; I venture to say no two readings would agree, and it is pretty safe to assume that each succeeding measurement would become less and less, as these fine edges were destroyed.

In the method described, using the inscribed circle, it is not necessary to have any edge whatever, as we may feel certain that the sides *would* be two inches if prolonged to meet each other.

For the purpose intended, it does not matter if these edges are blunt or even truncated slightly, as it is the known *position*, and not the actual length of the sides that is necessary.

In order to have the flat of the thread correct for such a pitch, in this case taking two inches as the base, the flat of which would be one

quarter of an inch or one-eighth of two inches, the method adopted is as follows:

The altitude of the frustum of this triangle was obtained after having removed the smaller triangle at the top, the sides of which are one quarter of an inch, by subtracting the altitude of this small triangle from the total altitude of the equi-angular two inch model, which gave the distance from the base to the top of this truncated triangle. By having this measured exactly, and the top and bottom planes parallel to each other, this distance came naturally, and evidently must be one-quarter of an inch without actually being measured. The actual measurement of this quarter inch flat would necessarily be very difficult, because we are dealing with the edges formed by obtuse angles, and the accuracy would certainly not be within the limit which would be required. After this triangle was established, a micrometer was made, which we have before us, in which the model two-inch triangle is used to determine the extreme limit through which the micrometer jaw shall move; establishing a "zero," if it may be so-called, for a starting point, the jaw of the caliper moving towards the smallest possible flat that could be measured, or that would be required for the finest pitches. This micrometer is, as its name implies, a divided circle and a screw, measuring very small advances of the jaw. In order to verify these subdivisions, lines were ruled by Professor Rogers, four hundred to the inch, with a diamond, upon the polished surface of the center of the bar.

There being 250 divisions graduated upon the index circle, and the pitch of the same being $\frac{1}{40}$ of an inch, each division represents $\frac{1}{10000}$ of an inch.

Each of the lines ruled 400 to the inch upon the sliding bar serves to check or correct the readings of every 25th division of the graduated circle, to provide corrections for possible errors in the screw.

By the use of this micrometer we can accurately measure the flats of the tools which are used to cut the United States standard or Franklin Institute thread of any number of threads per inch. In order to show the adaptation of this form of thread to interchangeable work, and also its extreme simplicity as a basis for an interchangeable system of gauges, we have before us (Fig. 2) a drawing showing how, should this thread be even larger in diameter on the outside, but with the diameter correct in the angle of the thread, the variation of this outside diameter from that of a standard cylindrical size has no effect upon the fit

of the nut which may be screwed upon the standard or upon a bolt representing this standard size. The only difference which we would notice is that the top of the thread would be narrower, and consequently the top would be higher, in the space cut away by the tap. Hence, taps that are made for tapping nuts of the United States standard thread, if made exactly right in the angle of the thread, that is, having the angle sixty degrees, and the diameter measured in this angle of the thread, correct, the outside diameter has no effect, within certain limits, to change its size, merely cutting away within the nut more metal outside of the limit of one-eighth the pitch. In the case of the bolt which fits this nut, the outside diameter should be kept

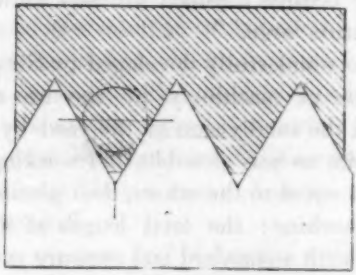

FIG. 2.

standard, the space between the bottom of the nut and the top of the thread of the bolt, allowing particles of dirt to lodge, without affecting the fit of the screw. This condition is often applied in the manufacture of taps, and has been found to lengthen the life of the tap in a very marked degree.

In the case of one company I have in mind, and who make small bolts and nuts, the taps they use being about three-sixteenths of an inch in diameter, they were formerly satisfied to have a tap cut fifteen or sixteen thousand nuts before perceptible wear occurred, they have found that in having them made in the way just mentioned, instead of stopping at sixteen thousand, they now cut a hundred and twenty thousand without practical variation in the size of the nut as compared with the standard gauge.

As an instance of the "eternal fitness of things," allow me to quote from Mr. Forney's Report* at the Convention of Master Car Builders, held in this city, in June, 1882:

* Report of the Committee of the Master Car Builders' Association, appointed "to investigate and report on the present construction of screws

"It is worthy of note that a remedy for the evil complained of by master car builders, that nuts made by some firms, or at some shops, would not screw on bolts made at others, at first baffled the ability of the most prominent manufacturers of tools in the country, and to provide an adequate remedy it was necessary to secure the assistance of the highest scientific ability in the country, which was supplied through the co-operation of the Professor of Astronomy of the oldest and most noted institution of learning in the land.

"The man of science turned his attention from the planets, and the measurement of distances counted by millions of miles, to listen to the imprecation, perhaps, of the humble car repairer, lying on his back, and swearing because a $\frac{5}{8}$-inch nut—'a leetle small'—will not screw on a bolt a 'trifle large.'"

In the system so wonderfully developed by Sir Joseph Whitworth for the manufacture of machinery by the use of interchangeable gauges, he obtained the subdivision of the yard by making three foot pieces as nearly alike as was possible, and working these foot pieces down until each was equal to the others, then placing them in his millionth measuring machine; the total length of the three foot pieces was then compared with a standard end measure yard.

These three foot pieces were ground until they were exactly equal to each other, and the three added together equal to the standard yard. The subdivision of the foot into inch pieces was made in the same way. This method necessitated extreme care, and also an enormous amount of time. In the method which has been adopted by the Pratt & Whitney Company, the sizes are not constructed in this way, but ruled lines, which represent the subdivisions of the British yard, are first investigated and found to be either accurate, or their corrections are applied, before a single gauge or any end measure is made. This method of investigation you will remember was described partially in our previous lecture.

One can readily understand what an unsatisfactory way it would be to attempt to subdivide a yard, or even a foot, into end measure pieces varying by sixteenths of an inch, say from a quarter of an inch to four inches, sixty-one in all, which would fulfill the condition of being

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and nuts used in cars; and the amount of accuracy that is desirable to secure, and the best means of maintaining it, in the standard adopted by the Association, in Richmond, Va., June 15, 1871," etc.

Submitted at the annual Convention, in June, 1882.

exact aliquot parts of the standard yard, which they each should represent. We can imagine the difficulties to be overcome by any one attempting this work by the subdivision of a standard foot, using the method adopted by Whitworth in 1834. Without having a line measure to which to refer, this standard foot—providing it was standard at the start—would necessarily be subdivided into two parts, each representing six inches, equal to each other of course, and together equal to the foot. Constant reference would have to be made to the standard foot piece, which would obviously result in more or less wear of the end surfaces. Then the six inches would be halved, and so on until the inch was obtained. Then, in order to prove that the inch was one-twelfth of the foot, it would be necessary to make twelve of these inches, or six inches equal to the six-inch piece, and the sum of all to be equal to twelve inches, or the original foot. We can all of us realize what has occurred to the standard foot in the course of this constant reference to it as the original standard.

Providing, even, that all these subdivisions were carefully made, and that no wear perceptible had occurred to the original standard, we are still not below an inch.

Subdivisions into quarters and sixteenths would still further complicate the matter. When the subdivision was complete, providing the operator's patience and life held out, he would then not be positive that he had even the inch a standard, having by this time worn out his original foot during such a long and tedious process.

Hence a line measure is really the only means of preserving this constant and standard quality for size, and it is this principle which has been the means of producing results, which so far, seem to fulfill all the requirements for an accurate system of interchangeable gauges.

In order to help out the matter, recourse must be had, for further subdivisions, to the use of a screw and a divided micrometer index circle. Just here we introduce the use of what has long been considered one of the impossibilities to be obtained by mechanical skill—a perfect screw. It was this that Whitworth was obliged to depend upon in obtaining his subdivisions by sixteenths. We have before us upon the screen a perspective view of the celebrated Whitworth millionth measuring machine, designed and used by Sir Joseph Whitworth to measure minute differences of inch standards. This machine, as you will see, combines the use of a screw for obtaining slight advances of the measuring faces of the instrument, a divided micrometer circle

and also a worm wheel to still further reduce the value of each division upon this carefully constructed micrometer.

We have now a section view of this machine, showing the method of providing against back lash of the nut and screw, which is secured by a double nut, as shown (Fig. 3) in the drawing before us. You will notice that the machine is very massive, and the accuracy with which it was constructed is designed to indicate with extreme delicacy differences between any two standard inch pieces, so called. Between the movable end of the rectangular bar which advances by means of the screw and nut, and the standard end measure piece, is a small polished piece of steel, having parallel faces, called a "feeling piece." The difference in length of two pieces is detected, it is claimed, by the variation in the reading of the divided wheel, and the uniformity of contact is indicated by means of this feeling piece.

The tightness of an end measure inch only one millionth of an inch

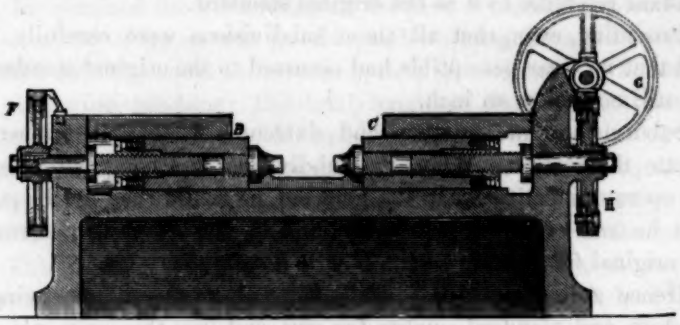

FIG. 3.

longer than one to which the machine had previously been adjusted, will, it is claimed, prevent this feeling piece from dropping when placed between the caliper jaw and the standard.

In order to make gauges for shop use, and to make them in such a shape as to be practical and not readily worn through constant reference, Whitworth proposed a form of cylindrical gauges represented by plugs and rings. These standard plugs he measures in his machine, duplicating his end measure sizes in this more practical form.

In using his measuring machine, he does not claim it to be an instrument for originating sizes, but merely for comparison of minute differences. Hence in order to maintain a constant standard, reference must be had to end measures which are certainly liable to sustain some

slight change from wear or oxidation. In the method used by the Pratt & Whitney Company, and which was proposed originally by Professor Rogers, the system adopted is that of making gauges to correspond to the lines which are accurate subdivisions of the Imperial yard, thus removing this liability to wear.

The gauges are made by referring each separate standard to a line which is ruled upon *hardened steel*, which has a rate of expansion the same as that of the hardened steel gauge with which it is compared. In making any number of gauges of the same size, this method will ensure the last gauge being exactly the same as the first, without reference to each other or to any other perishable standard. This has actually been done in the work so carefully gone through by the company, and it is possible and entirely practical to produce gauges so nearly alike by this means, that a variation between any two,

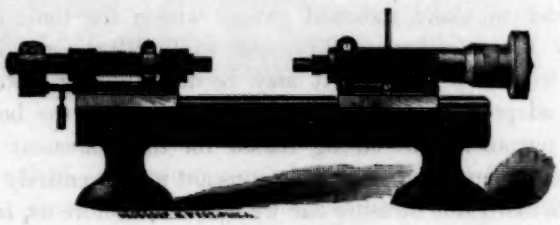

FIG. 4.

of even one forty or one fifty-thousandth of an inch can be discovered. We have found from our own experience that tool makers are very critical. They work closer than they themselves imagine, and in duplicating parts of any machine or any work requiring this exactness, they work often within a fifty-thousandth of an inch without being aware of the fact; so that in making a number of gauges of the same size, it is certainly necessary that they should be made within this limit. Nothing could throw more gloom over the spirits of a manufacturer of guages than the discovery that a tool maker is able to prove that two gauges, both marked alike, are unlike in size.

In the illustration before us (Fig. 4), we have a form of a simple bench micrometer or measuring machine, in which the screw and subdivided index circle form the main features.

In order to obtain practically the same result in duplicating sizes from a standard for ordinary gauge work, an auxiliary set of faces or

caliper jaws are used, and are shown at the extreme end of the instrument. These auxiliary jaws serve to hold a small cylindrical plug, so that in adjusting the machine or caliper to any given size, the pressure between the caliper jaws in which this standard is placed can be determined by the tightness of this small cylindrical plug. By taking the reading on the micrometer and bringing a second gauge in place of the original, the same conditions of pressure upon this second gauge may be readily determined, by noting the behavior of this little "feeling piece," as Whitworth might call it. The variation may then be read in the ordinary way by the subdivisions upon this divided index circle. As an instrument for originating a size, even with a screw of the utmost precision, it could not be expected to be infallible; but to copy or duplicate sizes it has been found to be very serviceable. A variation as minute as one hundred-thousandth of an inch has been shown to be appreciable in a case which has come under my own observation.

In order to make standard gauges within the limit of accuracy necessary for interchangeability, and to fulfill the requirements of modern workshop practice, it may be unqualifiedly stated that *line measure*, adapted for use as a practical reference, is the best standard for this purpose. The strong reason for this statement is that the ever present element of wear from constant use is entirely eliminated.

The standard line measure bar we now have before us, is one which has certainly shown this to be not only a strong reason, but a valid one. The lines which represent aliquot subdivisions of the Imperial yard, were ruled upon a dividing engine constructed by Professor Rogers, the work being done at the factory of the American Watch Company, at Waltham.

The total length, represented by the defining lines, is *exactly* one-ninth of the length of the Imperial Yard, or four inches, having no correction at 62°F. In other words, it is within a limit of $\frac{1}{100,000}$ of an inch. The subdivisions are inches, half inches, quarters, eighths, and sixteenths along one edge, and a band of lines, 2,500 per inch extending two inches from one end. Next is ruled a series of lines representing the bottom diameters or "tap sizes" of all United States standard thread gauges from $\frac{1}{4}$ to 4 inches inclusive.

Along the edge opposite the series of sixteenths, is ruled tenths and twentieths of an inch, covering a space also of two inches. This bar is made of steel, hardened and ground perfectly plane on its upper surface and highly polished. The graduated lines were transferred to

this surface using a *metric* screw, the pitch of this screw being one-half a millimeter. The ruling was done with a diamond. So carefully was the relation between the pitch of this metric screw and the length of the yard determined by Professor Rogers, that upon investigation, using the method of the "stops," mentioned in our previous lecture, the errors were found to be within $\frac{1}{50,000}$ of an inch for the particular subdivision of the Imperial yard which each represents.

When we realize that the transfer of each separate line, except the band of 2,500 per inch, was an actual computed setting of the diamond before the lines were traced, some idea may be obtained of the wonderful precision of the mechanism of the dividing engine, as well as the accuracy of the mathematical calculations involved.

Being hardened steel, the measurements of hardened steel gauges by

FIG. 5.

being referred to it, becomes entirely practicable at any convenient temperature, providing, of course, that an equal temperature for both standard bar and gauge is maintained. As the lines are less than $\frac{1}{25,000}$ of an inch in width, all comparisons must be made using a microscope.

The practicability of "calipering" under a microscope has long been urged by Professor Rogers as being the *only* exact method of inspecting standard gauges. The result obtained by use of this method, combining as it does, science and practice, has demonstrated beyond any question, the simplicity, as well as the accuracy of the method. To give some idea of its value for the purposes of *originating* standard sizes, an instance in mind may be stated.

A number of cylindrical size gauges, external and internal, a representation of which is shown in (Fig. 5), commonly called plugs and rings, were made. They were finished to agree with the subdivisions upon this little hardened steel line measure standard. Nearly eighteen months afterward, a new lot of the same sizes were made, and upon trial it was shown that any ring of the first lot fitted perfectly any plug of the second. Both lots had been made without reference to any

intermediate standard set of plugs, except to "rough them out," as it is called, within about $\frac{2}{10000}$ of an inch, all finishing after this having been done from data determined by calipering under the microscope.

A good gauge fit is not that the ring shall slide freely over the plug without perceptible "shake," but one such that the ring, when well lubricated with sperm or other *good* oil, shall move easily after having it fairly on the plug, showing no tendency to "grip" the plug while the ring is kept moving. Let the ring, however, stop moving even for a few seconds, and this condition of an apparently easy fit is suddenly changed to a *driving* fit, often causing serious damage to the gauge in separating them. In order to show this condition of perfect fit to best advantage, the temperature, of course, must be the same for both. The surfaces of the plug and ring must be as hard as steel can be made, and polished as carefully as the state of the art will admit. A good way of testing the accuracy of any set or pair of cylindrical gauges in reference to their being aliquot parts of any adopted standard, is to place within a ring which fits a standard plug, two smaller size gauges, tangent to each other, and if their sum is equal to the diameter of the larger single gauge, they will be tangent to the ring also. If exactly right, they will be found to hold together tightly, as the elements of cylinders which are in contact must either occupy the same space or be compressed enough to allow this practical tangency to be made. Care must be taken not to force the second gauge in too far, as this would evidently tend to injure them.

In the gauges before us, which are $2\frac{1}{4}$, $1\frac{1}{4}$ and 1 inch, we may see how nicely this test is met. If we use a gauge which is $\frac{1}{10000}$ of an inch smaller than one inch in diameter, the tangency is incomplete, for this gauge drops through, hardly touching.

A thousandth of an inch, you may say, is almost not worth considering, but here we have a standard plug and ring, the ring fitting perfectly, as you see. We now insert the plug, which is only $\frac{1}{10000}$ of an inch too small; it can be literally thrown on or off, one might even say that it "fairly rattles," the difference seems so great as compared with the fit of the standard.

We have here a $\frac{3}{4}$-inch plug, it is only $\frac{3}{10000}$ of an inch smaller than the standard, the plug and ring representing which we also have. You will notice it is not so loose as was the inch plug $\frac{1}{10000}$ small, but still one-third of this is perceptible, and shows plainly that it does not perfectly fit the ring.

In our experience in the manufacture of standard gauges, even this test is not a delicate nor a satisfactory one. It is not equal to that obtained by the use of a fixed caliper gauge having polished parallel jaws, a specimen gauge of this form we have before us (Fig. 6).

For the purpose of testing the larger sizes, this form of gauge is the best, as the friction between the two surfaces is a variable quantity depending upon the degree of hardness and polish of the fitting surfaces of plug and ring. It is possible, also, that the cohesive force

FIG. 6.

which we have mentioned, may act in this close-fitting relation, explaining why the ring should suddenly be so tightly "gripped."

With a two-inch gauge, a variation of $\frac{1}{20,000}$th of an inch is imperceptible, when a ring is used to determine this small difference, while with a caliper made as just described, having polished parallel jaws, this minute difference may be readily detected, if the caliper be first carefully adjusted to a standard two-inch cylindrical gauge.

To convey some idea of the minute variation which may thus be detected, I may state that a fragment of gold leaf, so thin that a mere touch of the fingers caused its total disappearance, on being carefully measured under the microscope, showed that its average thickness was $\frac{1}{30000}$th of an inch.

This same gold leaf would actually float in the air like a spider's web, and yet this extreme "thinness," if it may be so termed, is actually twice the limit of error within which it is possible to duplicate standard plug gauges, referring them to a line measure under the microscope.

The lines ruled upon this standard line measure bar in the space covered by the first two inches, 2,500 per inch, if placed one inch apart, the lines being magnified in proportion, would be represented by furrows or marks one-tenth of an inch wide, and would extend over a length of 416 feet 8 inches, or nearly one-twelfth of a mile.

The measurement of the diameter of drawn wire has long been a matter of confusion, owing to the use of numbers to designate arbitrary sizes which in many cases do not correspond with each other for the same numbers used in different standards or styles of fixed wire gauges. Even wire gauges of the same standard do not agree with each other, due perhaps to wear, if not from actual variation when new. To overcome this serious difficulty, the use of the micrometer, indicating thousandths of an inch for wire and sheet metal measurement, was adopted by the Association of Master-Mechanics, in Convention at Niagara Falls, June, 1882.

Since this date, in England, the Standards Department, Board of Trade, has issued a table of wire gauge sizes which are to be the legal standard on and after March 1st, 1884 (which, by the way, is to-morrow). In the table just mentioned, the numbers are retained, but each number shall represent exactly a certain diameter in thousandths of an inch. The table is also extended to include the metric system by placing opposite each size in thousandths of an inch, its value in millimetres, carried out decimally to tenths of a millimetre.

This table, for instance, begins with No. $\frac{7}{0}$, which is ·500 of an inch in diameter, or 12.7 millimetres. No. 1 is ·300 of an inch, and No. 50, the smallest in the list of sizes, is ·001 of an inch. The range, we notice, is from one-thousandth of an inch to one half of an inch. The variations are irregular, not advancing by equal amounts for each succeeding larger size. This is no doubt due to the effort to retain as nearly as possible a general average of the old wire gauge sizes. In every case, however, the exact size is stated in thousandths of an inch. The feeling in regard to the great lack of a uniformity in wire gauge sizes under the old notched gauge system, may be best expressed by

a remark recently made by the master mechanic of one of our best Eastern railroads.

He said that any one would be as likely to go to a lumber yard and order a plank ten feet long, twelve inches wide, and as "thick as a notch cut in a fence post made by Tom Jones," as to think of ordering sheet metal, specifying that it should be simply "No. 13 wire gauge," as has often been done, not even stating by what gauge it is so called.

The application of standards of length to ordinary workshop practice has so wide a range that it would be impossible, in the time at our disposal this evening, to attempt an enumeration of the many forms of gauges and templates necessary to secure the three important elements we have already mentioned, cheapness, serviceableness, and quantitative accuracy, even in a single department of work requiring interchangeability of parts, as for instance the manufacture of sewing machines, or the results obtained by the use of standard gauges in the manufacture of firearms.

It must not, however, be understood that all work produced is as perfectly in duplicate as are the gauges to which they are referred.

The guages are the means provided for keeping within bounds in the production of thousands of pieces of the same size or shape, in which oftentimes a certain amount of variation is allowed, both plus and minus.

Standard gauges prevent the gradual slipping away from the original size, and serve to bring back within the limit, variations of size, which would cause endless trouble and no small loss in the final assembling of these intended interchangeable parts.

This accurate fitting is only really necessary in gauge work, for if bearings or other parts of machinery were so closely made they would not move, or if by applying power enough they should be started, the absence of oil and the effect of the cohesion, if we may be allowed to say it, would quickly ruin the surface in contact.

A certain amount of "looseness" must be allowed, and by making the journals and the bearings in which they run, to certain definite sizes for each, the journal as many thousandths or ten thousandths of an inch smaller, as the size or length of bearing may require, referring each to some particular gauge as a standard. This being done, no fear may be entertained that other than a satisfactory fit may be the practicable result.

Before concluding, brief mention should be made of the efforts to

secure uniformity in gauge dimensions for steam and gas pipe thread fittings; a standard for which is now claiming the serious attention of manufacturers and users of pipe and pipe fittings. Pipe thread dimensions, when permanently established in the form of standard gauges, made so by the use of accurate subdivisions of the Imperial yard, though seeming to be an unnecessary refinement for so ordinary a class of work, really furnishes the means of extending the already well developed and recognized principle of modern manufactures, and which is "cheapness, serviceableness and quantitative accuracy."

Isolation of Calorific Rays.—F. van Assche places a drop of distilled and melted selenium upon a strip of glass, which is immediately covered by another plate of thin glass and the drop is compressed so as to spread it uniformly in a very thin, homogeneous layer. Care should be taken to avoid boiling the selenium upon the plate, as it would produce vapors, which, in condensing, form cells containing particles or crystals of selenium, between which light can pass without decomposition. When these precautions are taken, the chemical rays are reflected and the luminous vibrations are converted into electric energy; the calorific rays alone traverse the plate, after having undergone a certain amount of refraction. When the plate is heated to + 250° (482°F.), it appears to convert all the radiations into obscure rays. It can be utilized in various ways in the analysis of calorific rays: for example, in the experiments of Nobili and Meloni; in the photographic chamber, as an isolator, extinguishing all luminous and chemical radiations; by oculists, to shut out the rays of medium and of great refrangibility; and in the analysis of solar and lunar radiations. The coefficient of transmission is positive for heat, 0 for light, negative for the chemical rays. The greater the intensity of ordinary light the more completely it is suppressed by selenium.—*Comptes Rendus*, Oct. 15, 1883.

Interior African Sea.—Many persons have supposed that the French Commission, which was appointed to examine de Lessep's project, condemned it, but this is a mistake. The Yellow Book, published by the Ministry of Foreign Affairs, shows: 1. That the exactness of the scientific labors on which the project is based is beyond all question. 2. That the execution of the canal which is to be the

feeder of the future sea presents no difficulty. 3. That the work would be durable, since, even if we admit the most unfavorable hypotheses with regard to evaporation and saturation, the sea would have an assured existence of more than one thousand years. 4. That in no point of view could the sea be injurious, but that on the contrary, it would favor the development of colonization, by ameliorating the climate, diminishing malaria, and increasing the fertility. 5. Opinions have been divided as to the importance of the new route which would be opened to commerce, to the industry and security of Algeria; however, no one has been able, from any of these points of view, completely to deny the utility of the submersion of the basin of the Chotts. General Favé and others have eloquently set forth the capital importance of the interior sea, as well in a colonial as in a military point of view.—*Comptes Rendus*, April 16, 1883. C.

Propagation of Explosive Waves.—Berthelot and Vieille give the equation

$$\theta_1 = \theta_0 \sqrt{\frac{Q+q}{q}}$$

in which Q is the amount of heat set free at the moment of chemical combination; q, 273 times the specific heat; θ_1, the velocity of explosive translation of gaseous molecules; θ_0, the velocity of mean wave translation after the explosive wave has ceased to exert any influence. They have verified the formula, approximately, for a score of gaseous mixtures of very various composition. They think that in the act of explosion a certain number of molecules are thrown forward, with all the velocity corresponding to the maximum temperature developed by the chemical combination. This movement is transmitted from one inflamed edge to another, in a wave which is propagated with a velocity either identical or comparable to that of the molecules themselves.—*Ann. de Chim. et de Phys.*, xxviii. 293. C.

Tanning Linen.—A Belgian inventor, M. Piron, has invented a method of rendering cellulose tissues impermeable and very durable, without injuring their flexibility, and without much increasing their weight. By examining the bandages of the Egyptian mummies he inferred that the best preservatives would be found in the vegetable kingdom, and he has given preference to the green tar of birch bark, which furnishes the perfume of Russia leather. The tar forms, with alcohol, a solution of great fluidity; but when once dried it becomes

resinous and resists the solvent power of alcohol. It can be combined with the most brilliant colors. These qualities enable it to penetrate the capillary vessels of tissues, covering them with a varnish of great elasticity, which resists the corrosive action of acids, sea water and changes of temperature. The density is very small, so that the tissues are made impermeable with a slight increase of weight. The prepared stuffs can be folded without scaling. The aromatic odor drives away insects. Microscopic vegetation cannot grow, because neither air nor water can penetrate into the interior of the fibres. The invention can be applied to all vegetable tissues, such as sail cloths, cordage, awnings, curtains, etc.—*Chron. Industr.*, April 22, 1883. C.

Interior of the Globe.—Most of the mathematical investigations upon the figure and interior constitution of the globe, start from the hypothesis that it is entirely fluid, with the exception of a comparatively thin, superficial crust. Prof. Roche, of Montpelier, has shown that the hypothesis of complete fluidity cannot represent, within the limits of probable error, the polar flattening and the constant of equinoctial precession. He finds, however, that both of these facts are consistent with the hypothesis of an interior nucleus, with a density of about 7, and an external layer, with a thickness equal to $\frac{1}{6}$ of the semidiameter and a density of 3. This would give an interior mass with a specific gravity corresponding to that of meteoric iron, while the enveloping layer would be comparable, in density and constitution, to the stony äerolites.—*Comptes Rendus*, April 23, 1883. C.

Meteoric Dust.—While admitting that the brilliant sunsets may be partly owing to volcanic dust, Nordenskjold thinks that cosmical dust must also have played an important part. The snow which fell in the latter part of December, in the neighborhood of Stockholm, contained small quantities of black powder. Some of this powder was found upon analysis to contain carbon, which when burned left a reddish residium of oxide of iron, silex, phosphorus, cobalt and nickel. The quantity of cobalt and nickel amounted to half of one per cent. The Swedish Academy of Sciences has made an appropriation for conducting investigations, on a large scale, and at a distance from human settlements. Nordenskjold communicated this information to Daubrée, in the hope of awakening an interest which would lead to like investigations in the Alps, the Pyrennees, and the Jura.—*Les Mondes*, Feb. 2, 1884.

Liquefaction of Gases and Solidification of Bisulphide of Carbon and Alcohol.—Caillettet, in a note published in 1882, recommended liquefied ethylene as a means of obtaining an intense cold. Wroblewski and Olszewski have employed a new apparatus, which allowed them to submit considerable quantities of gas to pressures of several hundred atmospheres, so as to congeal bisulphide of carbon and alcohol and completely liquefy oxygen with great facility. By boiling ethylene in a vacuum they have obtained a temperature of — 136° C. (— 212·8° F.) Liquid oxygen is colorless and transparent, like carbonic acid; it is very mobile and forms a very marked meniscus. Bisulphide of carbon freezes in the neighborhood of — 116° C. (— 176·8° F.) and melts in the neighborhood of — 110°C. (— 166° F.). Alcohol becomes viscous, like oil, in the neighborhood of — 129° C. (— 200·2° F.) and solidifies in the neighborhood of 130·5° C. (202·9° F.), becoming a white body. Liquefied nitrogen is colorless, with a visible meniscus.—*Comptes Rendus*, April 16, 1883.

Book Notices.

HINTS ON THE DRAINAGE AND SEWERAGE OF DWELLINGS. By Wm. Paul Gerhard, Civil Engineer. New York: W. T. Comstock, 1884.

Again we are called upon to chronicle the advent of a new work on sanitary science; and that before us is well calculated to impress one with the magnitude that the branch to which it is devoted has grown.

In the first chapter the author quotes from an English report on filth diseases in which the following passage occurs: "Whether the ferments of disease, if they could be isolated in sufficient quantity, would prove themselves in any point odorous, is a point on which no guess need be hazarded; but it is certain that in doses in which they can fatally affect the human body they are infinitely out of the reach of even the most cultivated sense of smell, and that this sense (though it's positive warnings are of indispensable sanitary service) is not able, except by indirect and quite insufficient perceptions, to warn us against risks of morbid infection."

Such remarks would be superfluous were it not that the absence of odor appears to be regarded by the majority of persons as indicative of freedom from danger, and that the danger is proportioned to the offensiveness of the smell; apparently ignoring the fact that where persons

catch the most virulent diseases they have no direct intimation of danger until the symptoms manifest themselves. It is also to be remembered—even if the sense of smell were admitted to be reliable to detect dangerous emanations—that this sense is subject to the same inequalities in different individuals as the other senses, and is less likely to have the attention directed to its deficiency than in the case of sight or hearing.

Among the numerous methods and devices illustrated in the work it is impossible to decide at present upon the most effective, no doubt many will be found to be inefficient; a practical test under varying conditions being necessary in many cases to demonstrate their defects; but it is probable that some possess nearly equal merits, and among them there will result a sharp competition for favor.

As illustrating the recent developments of this comparatively experimental stage of sanitary appliances, the work under consideration is valuable. W. B. C.

THE AIR WE BREATHE AND VENTILATION. By Henry A. Mott, Jr., Ph.D., E. M., etc. Mott Series, No. 2. New York: John Wiley & Sons, 15 Astor Place. 1883. 81 pp.

This is a treatise upon the two subjects indicated in its title. The first part is a description, chemical and mechanical, of the atmosphere and its impurities, with some statements of the effect of the latter upon upon the health of the persons inhaling them.

The second part consists mainly of descriptions well illustrated with cuts of various patented devices for and systems of ventilation, in some cases evidently copied directly from the manufacturers' circulars.

The author lays special stress upon the advantages of ventilation by "aspiration," that is, of draining the foul air from the place to be ventilated, rather than of forcing fresh air into it, in which, on the ground of economy, a question which he does not discuss, he is no doubt correct; otherwise there does not appear to be much practical difference, for, if fresh air is forced in, the foul air must go out, and if the foul air is drawn out, fresh air will come in, and if the points of inlet under it are equally well placed the value of the result attained is not affected by the manner in which it is accomplished.

He advocates the placing of the exhausts near the ceiling rather than the floor, a point as to which there is a difference of opinion among good authorities. G. M. E.

List of Books added to the Library during October, November and December, 1883.

(Continued from page 232.)

Bureau of Education.

Co-education of Sexes. 1871. Washington.

Common Native Trees of Northern United States.

Comparative Statistics of Education in Sixty Principal Countries. Washington, 1882.

Educational Conventions and Anniversaries during 1876. Washington.

History, Work, etc., of Bureau.

International Conference on Education. 1876.

International Educational Congress. Brussels, 1880.

Medical Colleges in the United States. Washington, 1881.

Medical Education. 1874. Washington.

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From the Bureau.

Bureau of Ordnance. War Department, U. S. Notes, Nos. 317 and 320 and Index to Vol. 10. Washington.

From the Chief of the Bureau.

Bureau of Statistics. Treasury Department, U. S. Annual Reports of the Chief on Commerce and Navigation of the United States for 1837, 1838, 1845, 1846, 1848, 1881 and 1882.

Quarterly Reports of the Chief, Nos. 3 and 4, 1882–1883, and for three months ended June 30, 1883.

Summary Statement of the Imports and Exports of the United States, and Monthly Reports of the Chief of the Bureau, 1867 to 1883. From the Chief of the Bureau.

Burlington, Iowa. First and Second Reports of the Superintendent and Secretary of the Water Works. 1878–1882.

From the Superintendent.

Burritt, E. H. Geography of the Heavens. New York. N. D.

From B. B. McKinley.

Butler, J. D. Nebraska. Its Characteristics and Prospects.

Cameron, K. Plasterer's Manual. New York: Bicknell and Comstock 1879.

Canal Commissioners of New York. Annual Reports for 1843, 1847, 1854, 1855, 1859, 1860, 1862, 1863, 1867, 1869, 1873, 1874, 1876, 1877. From the Commissioners.

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Census of 1870. Vital Statistics.

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Chicago, Ill. Annual Reports of the Department of Public Works. 1879–1881. From Commissioner D. C. Gregier.

Chicago Public Library. Reports of the Board of Directors. 1872–1882. From W. F. Poole, Librarian.

Cincinnati, Ohio. Annual Reports of the Trustees of Water Works, 1851–1853, 1857, 1860–1863, 1867–1882. Also Report on Analysis of Western Waters. By Locke, 1853. Also Special Report on Extension. By Scowden. 1872. And Report of Board of Experts on Warden Compound Pumping Engines, March, 1879.
From the Trustees.

Cincinnati Board of Trade and Transportation. Fourteenth Annual Report. From the Board.

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Clayton-Bulwer Treaty and the Monroe Doctrine. Washington, 1882.
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Cleveland, Ohio. Twenty-seventh Annual Report of the Board of Trustees of Water Works for 1882.

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Concord, N. H. First to Eleventh Annual Reports of the Board of Water Commissioners. 1872–1882.
From N. C. Hastings, Superintendent.

Condit, C. L. Painting and Painters' Materials. New York Railroad Gazette, 1883.

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From Chas. H. Banes, Member of Councils.

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From the Board.

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(To be concluded.)